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PUBLIC HEALTH

PAPERS AND REPORTS

VOLUME XXVIII

PRESENTED AT THE THIRTIETH ANNUAL MEETING OF THE

American Public Health Association

NEW ORLEANS, LA.

DECEMBER 9, 10, 11 AND 12,

1902

COLUMBUS, OHIO:
The Berlin Printing Company
1903

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PRESIDENT'S ADDRESS.

By HENRY D. HOLTON, A. M., M. D., Brattleboro, Vt.

Members of the American Public Health Association and Friends—It is with pleasure that on this, the Thirtieth Annual Meeting, I can greet you in this southern clime, redolent with sunshine and the perfume of flowers still blooming in this closing month of the year, in this beautiful Crescent City, celebrated for its commericial activity and distinguished for a hundred years for its generous hospitality to all strangers who come within its gates. Those of us who come from the North, whether from the states or from the provinces, designated by Kipling as our "Lady of the Snows," bring the warmest appreciation of the beauties so manifest here, and our colleagues from our sister republic of the south will endorse all that can be said of the charms presented in this State, once ruled by the same Spanish sovereign as their own country.

Looking back to 1880 when we last met here, what changes have taken place, how great the progress in sanitation. Then the burden of your presiding officer's address was the "Education of the Public," in order that they might appreciate the needs of communities, states, and the nation, and seek to aid rather than hinder the work of health authorities.

The most wonderful progress has been made along all sanitary lines; much of it is the result of the work of this Association; then there were only twenty-two states having any recognized health officials; now all the states of the Union except four have some form of Public Health Department. Great progress has been made in the registration of vital statistics, although there is still much to be accomplished in this direction. While thirty of our states make a semblance of reporting them, only eleven states have adequate registration laws. In Mexico and the different provinces of the Dominion of Canada a good system of registration prevails. The Association should take measures that will lead to an increased effort being made to induce all states and countries on this continent to establish a Bureau of Vital Statistics.

For years germs of various kinds were recognized as organisms that gave rise to disease; it was not until 1881 when Koch developed his gelatine method of culture that bacteriology began to assume leadership. It is to workers in this department that the wonderful advances have been made in our knowledge of the causes of disease, and of the means of prevention. At the time of our meeting here there was only one laboratory in the world where instruction was given in bacteriology. This was in Copenhagen. Now many of the states and cities of this continent have well equipped establishments where constant bacteriological investigation is being prosecuted in the interest of sanitary science. Antitoxin for the prevention and cure of diphtheria has been developed, and its value in decreasing the mortality of this disease cannot be overestimated.

Within these years the discovery of pathogenic bacteria of most of the contagious diseases has given greater precision to diagnosis, and enabled health authorities to make more specific, accurate and in many instances less irksome quarantine regulations. It has been definitely determined that we possess various germicidal agents that have a definite value in destroying the various pathogenic organisms. Many communicable diseases whose appearance in any community were sure to result in terror, panic and enormous sacrifice of life, are now controlled by health authorities in so thorough and satisfactory a manner that the death rate is not perceptibly increased. Communities exhibit the greatest confidence that the health department will take care that the business of the people is not interfered with. It was for members of this body, and their associates, to discover and announce that the mosquito was the intermediate host which with swift wing bore the parasite of malaria and yellow fever. No more illustrious page in the philanthropic history of the world will ever be written than that made possible by the Sanitary Corps of the United States Army in the late Spanish-American war, brilliant as were the achievements of the army and navy, it will be written that their victories only made it possible for the American sanitarians with their energy and scientific attainments to clean the Capital City of Cuba, which for a century or more had been the foci of pestilence, destroying hundreds of thousands of the human race; always a menace to the commerce of the world and annually costing this country millions of treasure. Under the leadership of Medical Military General Leonard Wood, our lamented Vice President, Dr. Walter Reed, Dr. W. C. Gorgas, and their associates, not only cleaned and drained the city, but by following the suggestion made by Dr. Carlos J. Finlay, in 1881, determined by actual experiment that culex fasciatus was the original carrier of yellow fever. The patriotism of the military as they spring to the defence of their country, always deserves and receives the applause of the populace. Their deadly conflict on the battle field is made easy by martial music, the booming of artillery, the rattle of the infantry fire, and the advancing step of comrades. How much more should we recognize the great courage of such devotees of science as Drs. James

Carroll and Jesse W. Lazear, who, filled with a great philanthropic love for humanity, calmly, quietly, without the cheers or even the knowledge of the multitude, silently submitted themselves to the test to determine in what way this pestilence was communicated. We are told, "greater love hath no man than this, that a man lay down his life for his friend." We find that Jesse W. Lazear, fired with and impelled by this great love for his fellow man, did offer his body as a sacrifice upon the altar of scientific investigation, to the end that in the years to come hundreds of thousands might escape this pestilential death. The memory of his heroic example should fill all of us, and future generations, with this inspiring, ennobling love:

"Two paths lead upward from below,
And angels wait above,
Who count each burning life-drop's flow,
Each falling tear of love.

Though from the Hero's bleeding breast
Her pulses Freedom drew,
Though the white lilies in her crest
Sprang from that scarlet dew,—

While Valor's haughty champions wait
Till all their scars are shown,
Love walks unchallenged through the gate,
To sit beside the Throne!"

The practical result of all this work and sacrifice has been evidenced this past summer; not a case of yellow fever has originated in the island of Cuba for the past fourteen months, and the quarantine period has been shorter by three months; thousands of lives and millions of treasure have been saved, and a feeling of security has filled the communities of the southern portion of our country.

It becomes my painful duty to announce that the white angel of death has silently entered the circle of our membership, and beckoned several of our colleagues over the river to receive the reward of the faithful worker for the uplifting of his fellows.

It is with profound sorrow that we record the death, on November 23rd last, at the early age of fifty-one years, of Major Walter Reed, M. D., our First Vice President for the current year. A native of Virginia, he entered the army from that state in 1875 as assistant surgeon. Was a member of the commission to investigate the cause of typhoid fever epidemics in the army during the Spanish-American war. In 1900 he was detailed with Surgeons Carroll, Agramonte and Lazear, with instructions from the surgeon-general of the army, to "give special attention to the questions relating to the etiology and prevention of yellow fever." The result of their observations and experiments as given in a paper read before this Association at the Annual Meeting in 1900, was that "the mosquito served as the intermediate host for the parasite of yellow fever;" this announcement was received by the profession of the world with especial interest. At the time of his death he was Professor of Bacteriology and Pathology in the Army Medical School and lecturer upon the same subjects in the Medical Department of Columbian University, Washington, D. C. He was a cultivated, genial gentleman, whose death will be mourned by a large circle of personal friends.

Dr. Robert C. Kedzie, an ex-President of this Association, died November 7th last at the age of seventy-nine years. He was for forty years a Professor of Chemistry in the Agricultural College of Michigan. He served as surgeon of the 12th Regiment Michigan Infantry during the civil war. He was early in the field as a sanitarian, was a member of the State Board of Health of Michigan and for several years its president. An acquaintance says of him, "as an agricultural chemist, as director of the Government Experiment Stations, as an inspiring lecturer, public speaker and leader of useful thoughts, he was one of the foremost citizens of the state." With a power of achievement that was exceptional, he was the quiet, unobtrusive man who noiselessly did things. Justice, not fame, writes the fitting epitaphs of such characters.

Albert L. Gihon, another ex-President of this Association, died November 17th, 1901, at the age of sixty-seven. He entered the navy in 1855, as assistant surgeon, passed through successive grades to senior medical director. In 1895 he was placed on the retired list, having arrived at the age limit. He was decorated by the King of Portugal as a Knight of the Military Order of Christ, for services rendered the Portugese men-of-war and the colony at Dilly, on the island of Timon. He was the author of a text book on naval hygiene and of numerous essays on sanitary subjects. It is not necessary to speak in detail of the life and character of our departed friend; to know him was to admire his frank, manly ways. Energetic, quick to formulate, free to express his opinion, firm and tenacious in his views, pretenders and time servers met with no mercy at his hands; courteous and kind, a gentleman, a reliable friend whose memory will always be cherished.

Thomas J. Turner, A. M., M. D., Ph. D., Medical Director of the U. S. Navy, retired, died just before our last meeting, aged seventy-two. He entered the navy as assistant surgeon in 1853, passing through all grades to medical director and was retired by age limit in 1891. He was for a time Secretary of the National Board of Health. He was the author of many important contributions to current literature on sanitary subjects.

Col. John F. Kimball, M. D., of New York, a member since 1892, entered the military service as assistant surgeon of the 121st New York Volunteer Infantry in 1865, joined the regular army in 1867, and was assistant surgeon-general at the time of his death. One intimately associated with him says: "As a military surgeon and sanitarian Dr. Kimball took high rank as one imbued with the highest sense of duty." His personality was quiet, yet magnetic, his manner dignified but always approachable. As he found life waning he sought his home in the Catskills, and as the great panorama of plain, mountain and garden in which his life had been spent, faded from his earthly view, the greater panorama of a noble, pure and self-sacrificing life was opened to his spiritual vision, as on April 19th last he entered peacefully upon his long leave of absence.

One of our old members, Francis W. Lewis, M. D., of Philadelphia, was not often with us. Also George E. Tyler, M. D., of Denver, a younger member; now both passed to their eternal rewards.

It is with special sadness that we record the loss of our young colleague, Wyatt Johnston, who died on the 19th of June. He was an enthusiastic worker in sanitary science, especially in bacteriology; becoming a member of this Association in 1892, he at once manifested great interest in all its purposes, and evinced a desire to aid, so far as possible, along all the general lines of its work. It was from him, I think, that the suggestion first came for a Laboratory Section. This suggestion was most cordially received by all members, and the section established with Dr. Johnston as its first secretary. His interest and labors in the general meeting did not abate in the least. His modification of Widal's reaction of the typhoid bacillus is recognized and employed by scientists all over the world. Thus will his memory be forever preserved among scientists. It saddens us to thus take leave of fellow workers, but

"Let us be patient. These severe afflictions
Not from the ground arise,
But oftentimes celestial benedictions
Assume this dark disguise.

We see but dimly through the mists and vapors
Amid these earthly damps,
What seems to us but sad funereal tapers
May be heaven's distant lamps.

There is no death. What seems so is transition.
This life of mortal breath
Is but a suburb of the life elysian
Whose portals we call Death."

The avowed object of this Association being the suppression of contagious diseases, impels me to direct your attention to a class of diseases which has received but scant consideration from this body. I am aware that it is a subject that has, with few exceptions, been tabooed, especially in mixed audiences of non-professional people; if, however, we are to make any abiding progress in the control and prevent the infection of our communities and the degeneration of our posterity, all philanthropists, men and women, interested in promoting better sanitary conditions and in the suppression of vice, must join in a united effort to this end. That the nations of the world are arousing to the importance of the subject, is shown by the fact that in the recent congress held in Brussels, to consider the prevention of syphillis and venereal diseases, thirty-four countries were officially represented. Undoubtedly the first effort should be along educational lines. A marked step in advance was taken by the Belgian government in issuing instruction to the public on the prevention of these diseases. It demonstrates that it is possible to present this subject to communities without offending the ordinary sense of any. We must depend upon medical men to take up this work of education with all classes of society. Miss Blanche Leppington, of England, in a recent contribution on this subject, sums up the matter by saying, "One thing the doctors have pretty much in their own hands." In their personal practice and by the use of their public influences in their communities they can sow the seeds of the most valuable teaching. They can dispel the illusion that regulation affords a quarantine against disease; they can spread as no one else can, through all ranks and classes, a wholesome anxiety which will act to some extent as a check upon indulgence, and which will also tell immediately upon the partner of that indulgence. They can do better still; they can try if there is no allurement for young men in the ideal, recommended by sense and their physician, of unbroken chastity and pure marriage. The doctor who has done nothing to suggest or fortify such generous ideal in the minds of his male patients has not done his part toward the solution of this problem.

The great public should be thoroughly informed in regard to its prevalence, its degrading effects morally, intellectually and physically, that its transmission to successive generations is a prolific cause of the degeneracy leading to crimes which are alarmingly increasing. We need not at this time consider the fact that it prevails in every country in the world, nor the various ways in which it finds its way into all classes of society. We would, however, recommend that a committee be appointed to consider the whole subject of its eradication and the best means, educationally and otherwise, to accomplish this end.

We are annually appalled at the mortality from tuberculosis, although there has been a slight reduction in the last decade. The bacillus which causes this disease is everywhere recognized. For several years sanitarians and physicians have been impressing upon the people the fact that these bacilli may infect any who may be so unfortunate as to take them into their system; so effectually has this fact been impressed upon a large number of people that the sufferer from this infection is today almost an outcast; especially is this true if such person is without a permanent home or with but limited means; no one is willing to take him into their home; they shun him on the street, he finds himself a social vagabond. Sanatoria there are, but to gain entrance to these private homes one must have money in plenty. At present there are only three within our national domain that are in any degree an asylum for impecunious sufferers. We provide care, homes, attendance, and everything that is requisite to the proper maintenance of all our criminal classes and for our insane. We have hospitals for the poor, sick with any other disease. Yet our legislators and philanthropists hesitate to appropriate money for persons suffering from this plague. However, when we compute the amount now expended in the care, with the loss in wages, of those suffering with this disease, it appears that upon a commercial basis it would be an especially economic expenditure for the State. In the United Kingdom of Great Britain the majority of the forty-two thousand who die annually of pulmonary tuberculosis, are between the ages of twenty and forty. The loss of earnings of this number is estimated at between fifty and sixty millions of dollars annually, while there is expended in their care for the same period five millions more. With all of these facts before us it would seem that having succeeded in impressing the mass of the people with its communicability, we should take the next step in their education, which is the necessity of providing not only care and maintenance for this class of sufferers, but at the same time the best means of preventing others from being infected. Compulsory notification stands first and should be insisted upon. When it is fully understood that as soon as health officials are notified of the existence of this disease in any family, instructions will be given and enforced which will render it possible for the infected person to remain in the home without others becoming infected; then the infected one will not be an outcast branded for all to shun, and the great objection to notification is removed. When education has advanced a step further, and the public come to appreciate that sanatoria for tuberculosis opens to such as have unfortunately become infected, the most promising avenue of escape from certain death at the same time that it is an additional safeguard against the spread of the disease, then will the "vox populi" demand that justice be done the impecunious sufferers in the establishment by the state of institutions where in the earliest stage of the infection they may be placed and thus given all possible aid to recovery and the enjoyments of social and productive citizenship.

We believe that it is also the duty of the State to provide homes for those of this class, who from any cause have advanced beyond the period when recovery may be expected; thus soothing and comforting their remaining days and giving additional protection to the community. Is it not the duty of this Association to formulate an appeal to the different governments represented to establish such sanatoria and homes?

At the Congress on Tuberculosis held in Berlin in October last, Koch reiterated his statement of the previous year that it had not been satisfactorily demonstrated that human bacilli tuberculosis could be communicated to the bovines, and vice versa; hence it was unnecessary to use any measure to prevent the transmission of the disease from bovine to human. We have from Professor Brouardel the statement that in conversation with Dr. Koch that he admitted that there was a possibility of this transmissibility. On a previous occasion and in another presence, we took occasion to show that the concensus of opinion upon the propositions of the able and distinguished German savant was one of opposition; that by bacteriological, pathological and clinical evidence we were compelled to acknowledge that human tuberculosis had been communicated to bovines; that scientific observation in several cases demonstrated the fact that the human had been inoculated with bovine tuberculosis; this view is substantiated by Ravenel, Smith, Nocard, Arling, and many others. When we recall the fact that at the end of the year which has elapsed since Koch first made this statement of non-communicability, he has no new evidence to submit, while, as already stated, many other observers have produced evidence tending to establish the fact of intercommunicability, we believe it would be criminal to relax in any measure the efforts to prevent human beings from becoming infected from bovines.

The neglect of vaccination prepared the field for a general development of smallpox; all that was required was the seed. For the last four or five years there has prevailed an exceedingly mild form of this disease. In many localities the mildness of the disease has lead to errors in diagnosis, impetigo contagiosa, Cuban itch, elephant itch, but the most frequent error has been to call it chicken pox. The mildness of these cases results undoubtedly from the change brought about in our people by a hundred years of vaccination. This modified virulence and the reduced power of infection have been seized upon by anti-vaccinationists and the ignorant as an argument against vaccination. With great flourish of trumpets the mayor and chief health officer of Cleveland, Ohio, set up the claim that the city was free from smallpox, that this eradication had been brought about by a system of rigid sanitation, entirely without vaccination. However, within the following six months there were within that city 532 cases of the disease with 73 deaths. This was one-third of all the cases of smallpox in the State of Ohio, and more than three-fourths of all the deaths from the disease occurring in the State. This was the result of an attempt to stamp out smallpox in a populous city without vaccination. Everywhere we meet persons who have arrived at adult life who have never been successfully vaccinated, some from carelessness, others from simple neglect who are waiting for a more convenient opportunity, and others ignorant of the nature of this loathsome disease and its results, even in the present epidemic, and who are not properly informed or who have been misled by erroneous statements regarding vaccination. These persons make up a large and dangerous class in every community, who, when exposed to this contagion, contract the disease and become the foci from which a whole town or city become infected. The medical profession are familiar with and should frequently repeat the history of Porto Rico's deliverance from this pestilence. When the United States Government took possession of this island smallpox was epidemic, as it had often been before; no greater or more beneficent work in prevention of the disease has ever been done than that of Col. Hoff, the Chief Surgeon of the United States Army, who at once entered upon a work of compulsory vaccination of the whole population. A report of Major Azel Ames, makes the following statements:

"In October, 1898, smallpox was endemic in Porto Rico; in December it was epidemic; in January, 1899, it had "honey-combed" the island; by February there were over 3,000 recent cases, and the disease was spreading at a gallop. In February systematic compulsory vaccination, carefully and scientifically conducted and recorded, was begun simultaneously and with pretty equal efficiency in all parts of the island. It was vigorously prosecuted for *four months only*, till July 1st, when 860,000 vaccinations had been made in a population of about 960,000. Of these 87½ per cent were successful. The work then ceased, because completed; the disease had practically disappeared; the fuel for it to feed upon had been consumed by the 'head fire' of vaccination. In the two and a half years that have since passed, instead of the former annual average death rate of 621, the mortality from smallpox has been but two per annum in a population of nearly a million. Can any *honest*, intelligent person doubt in face of these indisputable and easily verified facts, *what* it was that in *four* short months drove smallpox from its wide and long time reign in the island, and has since kept it out? Vaccination alone did it, and will do it effectively wherever compulsory legislation, properly enforced, secures its benefits to all!"

The terrible results that may follow the general neglect of vaccination was shown in Germany, when a population which had allowed vaccination to be almost entirely neglected, had an annual mortality from smallpox of 15,000 to 20,000, culminating in 1871 in an epidemic in which there was 143,000 deaths. This aroused the Government to the importance of taking means to prevent such wholesale destruction of human life, and in 1874 a law was passed making vaccination compulsory in the first year of life and re-vaccination in the tenth year. With the enforcement of this act smallpox ceased to be a menace and the annual mortality since then, instead of fifteen or twenty thousand, has been on an average of 116.

These facts cannot be too often repeated; they should be advertised to the people in every hamlet on this continent. Only six of the states of this union have compulsory vaccination laws; eight may enforce it to check an epidemic of smallpox, and twenty-two may exclude children from school who cannot present a certificate of successful vaccination. The profession and public should be impressed that for one to be successfully vaccinated means that this apparently slight operation which most laymen think of so little moment that they would not hesitate to perform it themselves, should be done with the greatest care, that thorough antiseptic precautions should be taken, that the vaccinated person should be under the daily observation of the vaccinator in order that he may determine if proper characteristic vesicles form. Also to determine if the person is in a proper physical state for the operation, and to insure that the requisite antiseptic directions are observed. All virus should be submitted to the director of the state laboratory or public health service for bacteriological examinations as to its freedom from pathogenic germs. The strongest assurance may be given that if these precautions are taken there will never be any untoward results. With these precautions the state should require that every child should be vaccinated before it is two years old and re-vaccinated before the twenty-first year. Both state and municipal legislators should be made to carefully understand that only by thorough and successful vaccination can smallpox be prevented.

There are persons in almost every community who object to enforced vaccination, declaring that it is unconstitutional. However, in six states where this issue has been carried to the court of last resort, it has been pronounced constitutional.

For several years not only this Association representing the sanitarians of the country, but the American Medical Association as the representative of the medical profession as a whole, with various commercial organizations, have been making united efforts to secure from congress the necessary enactment to establish a commission of public health. At our meeting last year all the interests had apparently united under the "Spooner Bill," which had been before congress for several months, and which provided for the creation of a bureau, to be known as a National Commission of Public Health, in the treasury department, this commission to be under the supervision of a Commissioner of Public Health, who should be a physician and experienced sanitarian appointed by the President of the United States, or by the commission if such was possible, and an advisory council consisting of one member from each State and Territorial Board of Health, and from the Medical Corps of the Army, Navy and Marine Hospital Service. This Advisory Council was to meet once or twice a year in Washington. It provided also for an executive committee consisting of nine members of the commission, of which the commissioner, a representative from the Army, Navy and Marine Hospital Service, should be the ex-officio members, the remaining five members to be elected by the commission at the annual meeting, which should be convened by the commissioner whenever in his judgment public interests should so demand it, and this executive committee to have full power to act. It was expected that if a new department was created by congress it should have charge of the commercial interests of the country.

Various members of congress had assured the committee on legislation of this Association, that there was very little doubt that the proposed bill would become a law. However on the 19th day of December, 1901, Mr. Perkins, a member of the house of representatives from California, introduced a Bill into the House, "To increase the efficiency and change the name of the United States Marine Hospital Service." The effect of this was to divide the professional support which had been given the Spooner Bill, and in the opinion of the most experienced members of congress, to render the passage of either of these bills improbable, at the same time remarking, "that as usual the medical profession were not able to agree upon what was most desirable. However, should they ever be able to agree upon a measure intended to accomplish the objects of the Spooner Bill, undoubtedly congress would enact it into law."

Such was the state of this matter when early in March last the Legislative Committee of the American Medical Association met in Washington, with representatives of this Association, to take into consideration and determine the wisest course to be taken to accomplish something for the advancement of sanitation and public health. After a full conference, we were forced to conclusion that both the Spooner and Perkins bills were doomed to destroy each other; that the only hope was in a compromise measure by means of which something might be inaugurated that would mean progress along the desired lines; hence the adoption of such amendments to the proposed legislative act was believed would accomplish this end. A further conference with Senator Spooner resulted in a substitute bill, which finally became a law, providing for a public health and marine hospital service for the United States. It authorizes the detail of a commissioned medical officer of the service for duty in charge of administrative divisions, namely, marine hospitals and relief, domestic quarantine, foreign and insular quarantine, personal and accounts, sanitary reports and statistics and scientific research. Also for an advisory board for the laboratory, consisting of three competent experts, to be detailed from the army, the navy and the bureau of animal industry, by the surgeon-general of the army, the surgeon-general of the navy, and the secretary of agriculture, respectively.

It further provides that there shall be a competent person at the head of the division of chemistry, zoology, and pharmacology. In reporting the bill the committee say the dividing line between the federal powers of quarantine in its relation to foreign commerce and interstate commerce, and the state power of quarantine and police regulation, in its relation to the public health, is undoubtedly real, but it is often very difficult of discernment; and, therefore, it has often happened that state authorities have felt their province invaded by federal authorities and federal authorities have insisted that the national function was invaded by state authorities; and out of it all has grown more or less of friction, necessarily detrimental to great public interests. No statute, of course, can change the power of federal authorities, as defined by the Constitution, or take away the sovereign power of any state in respect of these matters. We have been impressed with the conviction that in the general public interest some recognition of federal legislation of the state health authorities, in the way of consultation upon subjects of vital consequence to the localities, and as to the rules to be put in operation by both state and federal authorities in accomplishing the same end, would of necessity bring about better understanding, and a coöperation which would inevitably promote a fuller accomplishment of the great purpose desired by both the federal and state authorities.

The federal jurisdiction is exercised primarily in the interest of commerce. The local or state jurisdiction is exercised primarily for the protection of life and health in the communities, and, secondarily, in the interest of commerce. In the hope and belief that good results will come from it the committee inserted Section 7, which, briefly stated, provides:

First: That when, in the opinion of the surgeon-general, the interests of the public health would be promoted by a conference of that service with the state or territorial health officers, that he may invite as many of said health officers and quarantine authorities as he deems necessary to send delegates, not more than one from each state or territory and the District of Columbia, to such a conference.

Second: That an annual conference of the health authorities of the states and territories and District of Columbia, shall be called by the surgeon general; each state, territory and District of Columbia to be entitled to one delegate.

Third: That it shall be the duty of the surgeon-general to call a conference upon the application of not less than five state or territorial boards of health, quarantine authorities, or state health officers, each of said boards or quarantine authorities joining in such request to be represented by one delegate.

It also provides for the collection and publication of vital statistics upon blanks so prepared as to have the returns from the different states uniform.

The work of this new department was charged with a most important duty on the same day that the act creating it became a law, by the passage of another act by congress "to regulate the sale of virus, serums, toxins, and analogous products in the District of Columbia." Also to regulate inter-state traffic in said articles and for other purposes. It will be appreciated by all that this law, if properly executed, will give the country, absolutely pure and reliable products, and will establish confidence of the public in them, thus removing the great objection that may have existed against their use.

This Association as well as all who for so many years have been striving for the establishment of a public health service, are to be congratulated that at last their efforts have been crowned with success. While all that was desired has not been granted, yet a substantial and, in many ways, satisfactory organization has been effected, with an able and experienced organizer, and a member of this Association, at its head. We congratulate Dr. Wyman upon enlarged opportunities thus presented to him to develop upon a scientific basis, a service of varied functions which shall equal in original research and demonstration any similar service in the world. Our government is about to engage in the construction of a ship-canal across the Isthmus of Panama; thousands of men are to be employed in a climate to which they are not accustomed, and where the most unsanitary conditions prevail. The terrible mortality attending the building of the Panama railroad, which was said to be so great that every tie in the road represents the sacrifice of a human life, must not be allowed to occur in the prosecution of this new enterprise. The public health service will find in this an extensive field for the exercise of well directed, energetic, sanitary work, along a large variety of lines; the selection of camping grounds, where temporary villages will spring up, their proper sewerage, water supply, food, in fact everything that will contribute to the preservation of robust, healthy life, at the same time that all sources of pestilential poisoning are sought in advance and destroyed, including the insects which, with hasty flight, carry the microbes of death within their tiny forms.

We have no doubt that in the discharge of these duties the public health service will achieve results which will redound to the credit of sanitary science everywhere.

It was long ago established that pestilence followed in the wake of famine. The fuel famine resulting from the recent coal strike has been a serious menace to the public health. It is not our purpose to consider the causes which led up to it, but rather to its general effect upon the people of the whole country. Several weeks have elapsed since it nominally came to an end; yet in the cities and large towns there are a large number of persons who are not able to obtain sufficient amount of fuel to properly cook their food or to keep their apartments warm; the result must be to induce many acute diseases and aggravate chronic affections, each of which adds to the mental and physical suffering of these people and an increasing death rate.

Other industries vital to the life and health of the people are liable at any time to be the subjects of similar sociological disturbances which might threaten the food supply of various sections, which would result in actual starvation in many cases, while suffering and resulting diseases of the innocent people far from the state where the trouble originated would be beyond compare. Under such circumstances we believe the public health service should, without delay, in all cases affecting the health of the inhabitants of one or more states, inform itself regarding the exact conditions, and having determined the causes that menace the public health of the states, submit to the President of the United States a statement of the facts and recommendations as to the necessary measures to be adopted to remove the causes of such a menace. When the recommendations of the public health service have been approved by the President, he shall immediately take such action and institute such proceedings as may be necessary to remove or prevent further menace to the public health of different states.

Such legislation should be asked from congress as may be necessary to effect these purposes.

In this matter as in all sanitary work, the public should understand that the ultimate object is the amelioration of human environment and the prolongation of life and happiness, as each decade exhibits its progress we may rightly expect proportionately larger results.

ADDRESS OF WELCOME ON BEHALF OF THE STATE.

By Hon. J. Y. SANDERS, New Orleans, La., Speaker of the House of Representatives.

Mr. President, Ladies and Gentlemen of the American Public Health Association—Owing to the unavoidable absence of the Governor of the State of Louisiana, and at his request and in his name, I extend to you a hearty welcome to this State of ours. Banded together, as you are, for the promotion of public health on this continent, it is with a peculiar sense of gratification that we welcome you to our midst. We realize that your aims, your aspirations, and your ideals look to the betterment of humanity by seeking to alleviate some of that suffering all flesh seems born to bear, realizing, as we do, that you have banded yourselves together for this purpose. Gathered in this hall to-night are representatives from countries of this continent, whose object it is, by education and enlightenment and the dissemination of intelligent ideas, to rob disease of the burden and the terror that have ground down the person and heart of humanity.

Ladies and gentlemen, we welcome you to our state. May your meeting be fraught with much good to humanity, and if, when you shall have adjourned, by your labors one life has been spared to humanity, one tear of suffering has been wiped away, then indeed have you cause for congratulation, and Louisiana will glory in the fact that by holding this meeting within her borders some more of the serious illnesses of mankind have been lessened; some of the powers of those twin giants of despair, disease and death, have been shorn. Gentlemen, again I welcome you to this fair and beautiful city of ours, and I earnestly hope, trust and pray that when your convention shall have adjourned, it will bear such fruit that in the future it will bring much happiness to the millions of this world. (Applause.)

RESPONSE TO THE ADDRESS OF WELCOME.

By Dr. H. D. HOLTON, President.

Gentlemen—On behalf of the members of the American Public Health Association, I beg to assure you that we receive with great pleasure your cordial words of welcome, not only to this State, which was the first to have any semblance of a board of health on this continent, but to this city, and sit at the feet of your sanitarians, gathering from them knowledge which shall be of benefit to the whole world. We congratulate you, Mr. Mayor, that you represent a citizenship of such an enlightened and patriotic people that has enabled you to enter upon a work which is so vast as the sewerage of a city of this character. We bid you God-speed. We shall go home feeling that we have visited a beautiful city and a beautiful State. (Loud applause.)

OPENING ADDRESS.

By Rev. MAX HELLER.

This gathering confers distinction and benefit upon the place in which it meets. Our city is honored in being identified with the earnest, helpful work wherein you are especially engaged. Your papers, your discussions and deliberations, as we shall witness them, or as our wide-awake local press will reproduce them, are bound to awaken thought in those who are privileged to harbor your presence; their echoes ought to vibrate long, both in our civic and our private lives. Exhibitions, it has been claimed, are the landmarks of progress which record the successive stages of our onward travel; conventions of scientific workers are exhibitions for the exhibitor; they are halting places for the comparison of notes, the measuring of achievements and the spurring on to renewed effort. Such exhibits leave no solid memorials of architectural beauty; but the impetus they furnish, the labors to which they give rise may loom infinitely larger in the growth of civilization than any monuments of brass or palaces of marble.

There is a certain fitness in your selection of New Orleans for this important gathering. We stand here at the gateway of a continent which awaits its redemption, with its entrance into the fellowship of modern culture, at the hand of the sanitarian; we are but a short distance from the Gem of the Antilles, to which our country brought, with the boon of liberty, the most invaluable lessons in health. Here in New Orleans, contending against adverse conditions, we have realized the blessings of public sanitation in a progressive reduction of our mortality, which seemed, from year to year, to accentuate the value of sanitary improvement and to vindicate our much maligned climate. We still have much to learn; great as have been the sacrifices which New Orleans has cheerfully offered in the cause of public health, noticeable as has been the education and enlightenment with which the past years have awakened us to the momentousness of these public interests, we look to the coming years, we look in part to such influences as that of your convention to carry on a campaign of further education among the people, to arouse their civic and their individual conscience to the mutual care and mutual protection which, with increasing knowledge, they owe in increasing degrees to one another.

We often dwell upon the advantages which come to all of us through the wide connections and intimate contacts of the modern life. Distances have melted away and approach has been made easy; millions have flocked into city life and tens of thousands are harbored by single buildings; the contacts are not only closer, in our more active existence they have become multiplied; as society becomes more intricately organized, man moves ever nearer to his fellow-man. It is a strange dispensation of Providence that this nearness of man to man should breed dangers to their health against which it is not enough for the individual to protect himself by personal cleanliness, but which compel united action in the common interest of all, making us realize our absolute brotherhood, our mutual dependence, out of the very sympathies of our physical constitutions. The first great sanitary system, the Mosaic legislation, presents itself to us as born in camp life, and every subsequent civilization, as it gathered into large centers, was forced to adopt sanitary measures on a scale corresponding to its wealth and power. Our own civilization is accompanied by a growth of the cities, by a movement of population towards metropolitan centers, the extent of which is unparalleled in all the previous experience of mankind. It is the city which creates all the glaring contrasts of fabulous wealth and incredible poverty; which rears the princely mansion on the stately avenue and the reeking tenement in the slum. The city it is, too, which gives rise to the most serious problems of sanitation; for wealth and luxury can erect no barrier of safety against the brotherhood of the flesh, which faces millionaire and tramp with the same dangers.

Sanitary science becomes thus a sermon, preaching with the eloquence of inexorable fact the lesson of loving our neighbor as ourselves. As it teaches cleanliness, the careful selection of food and covering, the beneficence of sunlight and open air to the individual, so it impresses the indispensableness of public spirit with a menacing appeal which only the most reckless may disregard. Men may be disposed to seek solitude in the throng, to enjoy the golden fruits of aggregated labor in selfish seclusion; sanitary science teaches these that there is no privilege that has not its compensating responsibility, that nature's God-ordained household tolerate no artificial separation. Sanitary science thus points the way to a city of God, in the distant future, wherein health and beauty shall level every barrier between soul and soul.

The science of sanitation may be looked to for the solution of many an embarrassing problem. Whenever it shall rule supreme in our cities, poverty will lose much of its sting. One reason for the push and stress of materialism in our age is that refinement has made the spectre of poverty so horrid; it is not only the magic of wealth that has increased, but poverty has become more hideous and hopeless in its stewing hives. It is untidiness and unkemptness, the repulsive personal habit, coarse speech and gross behavior which makes poverty unendurable; there is no torture to the most refined spirit in plain foods, cheap material, modest environments; cleanliness and health will confer upon poverty a dignity of independence which may be a welcome freedom to many a large strong soul. Our emancipation from the thraldom of materialism may thus lie, in the first line, with the sanitarian.

When humanity was still in its swaddling clothes of ignorance it was thought that every famine was a punishment from God and that epidemics were special visitations that had been incurred through special sins. We are on a fair way to abolish famines completely by the swift ministrations of world-wide commerce; and in the last few years the labors of the sanitarian have succeeded, time and again, in setting bounds to the ravages of pestilence when it was threatening to gather the world in its dark embrace. You have taught us that God's laws are benevolent; that disobedience to them is sin; that we may redeem ourselves from suffering by a humble submission to nature's orders. You have done more than to relieve our bodies of danger and pain; you have taught us to see our Heavenly Father as our guide to self-conquered freedom. May your labors in the sunniest city of the sunny south be crowned with every success of solid achievement in the present and noble inspiration for the future!

REPORT OF COMMITTEE ON DISPOSAL OF REFUSE MATERIALS.

By RUDOLPH HERING, C. E., Chairman, New York City.

In pursuance of the plan begun at the last meeting of recording descriptions of and experiences with the means of refuse disposal as practiced in the chief cities of America, when papers were presented on this subject concerning the cities of Washington and Boston, the committee respectfully presents at the present meeting further papers on the same subject concerning Vera Cruz and Mexico City, by Dr. Manuel S. Iglesias; Montreal, through Dr. Lachapelle; New York, by Dr. George A. Soper; Buffalo, by Prof. Olin H. Landreth; Cincinnati, by Dr. Byron Stanton, and Providence, R. I., by Dr. Charles V. Chapin.

The value of these papers consists chiefly in showing the various phases of evolution which this subject is undergoing and the tentative work which is still being done here and there, and also in pointing out the most promising directions for improving this branch of public service.

These papers are briefly summarized as follows:

Dr. Iglesias furnishes a description of the street hygiene of Vera Cruz and Mexico during the last generation.

When Vera Cruz was confined within walls the conditions were quite satisfactory, but after the walls were removed and the growth of the city became rapid, street hygiene did not keep equal pace, and the present conditions have become very objectionable. Improvements are described which promise very satisfactory results. There is now being introduced a new water supply, new sewers, new pavements and better methods of refuse disposal, all according to modern precepts.

The City of Mexico is likewise being remodeled in accordance with present knowledge and practice, and a description is given of the advancement and reconstruction of the sanitary works of this city. The new drainage and sewerage works may be completed this year. The refuse collection is now made daily in suitably covered wagons. The disposal of this matter is at present outside of the city limits on dumps, in a direction opposite to that of the prevailing winds.

Dr. Lachapelle has contributed a copy of a report made by Dr. E. Pelletier, dated October 31, 1902, which contains a description of the practice followed at Montreal for the removal and final disposal of garbage and refuse. It contains a very complete and thorough exposition of the subject.

After the preliminary statistics, refuse is defined as being kitchen waste, rubbish of all sorts, and ashes. An approximate analysis is also given for summer and winter conditions, and a comparison made with similar refuse of other cities, indicating that there is not a very material difference.

The collection and removal are described in much detail. The receptacles are mostly old flour barrels, and are not generally satisfactory. They leak in summer, and in winter, due to freezing, allow the contents to be taken out with difficulty. Beveled receptacles with covers are recommended to be furnished at cost by the city. On account of the climate wood is better than iron.

The carts are of wood and have a capacity of at least one ton. The collection is made during the daytime, except the markets, from which it is made during the evening. The carts are not cleaned after use. Althogether, in 1901, 67,000 tons were collected and removed.

The dumps are an old ravine and quarries no longer used, to which the carts travel three and one-half to four miles. The refuse of the central and eastern part of the town is disposed of in this manner. In the western part the refuse is taken to a furnace, with a haul of about two miles, and is there incinerated. The furnace is modeled after the English pattern, and has sloping grate surfaces of small area. All the refuse is dumped into the furnace. It is intended, however, during the winter months, when the ashes make up about 60 percent of the entire refuse, to pass it through a centrifugal separator and thus remove the fine material. Experience has suggested a number of changes in this furnace from its original design, which all tend to simplify it.

In summer the refuse burns without the addition of fuel and without previous sifting. The fires do not go out, and are banked at night, with slow combustion. Complaints about odors do not appear to have been made. The quantity of refuse burned in 1901 was 13,659 tons, at a net cost of 39 cents per ton.

The author does not favor the continuation of the dumping of garbage, and advises the building of more incinerators.

The report finally covers the subject of the removal of dead animals, advising the installation of a "knackery" yard, and makes some further recommendations concerning the removal of stable manure.

Dr. Soper has presented a report on the disposal of the refuse of New York City. He gives an historical account of the practice to date, indicating the progress made from the organization of the first department of street cleaning in 1881 up to its administration by Col. Waring, and of its subsequent decline, concluding with the promise of the present commissioner, Dr. Woodbury, to solve the problem of the final disposal of the refuse if possible. Dr. Soper describes the present organization, which is one of the best in the country, and also the property of the department, such as horses, carts, stables and scows.

The refuse is divided into three classes: Garbage, defined as materials any part of which has been used or intended for use as food; dry ashes, including broken glass, grass, metals, etc., and dry refuse, composed of sweepings, paper, wood, rags, etc. Different carts are used to collect these classes, and there are different intervals of time of collection.

A description is then given of the final disposal of the entire refuse, which amounts to about three million cartloads per annum, about ten percent of which is garbage. The latter is estimated as being about 147 pounds per annum per person. The garbage of Manhattan and Brooklyn is taken to Barren Island and converted into saleable products by the New York Sanitary Utilization Company, at a cost for Manhattan of $232,000 per annum under contract for a period of five years, beginning August, 1901. The contract for the treatment of the Brooklyn garbage at the same place was let for $47,990 per annum, to continue five years, from June, 1901.

Barren Island is 25 miles by sea from the southern extremity of Manhattan. The process of reduction is by cooking the garbage for eight hours, then pressing and extracting the grease. The residue is used as a basis for fertilizer.

Complaints have been frequent of offensive odors emanating from the works, chiefly after the cooking, which odors, however, could be removed by passing the air through a furnace at an expense which the company seems as yet unwilling to undertake.

In the boroughs of Queens and Richmond there are five crematories. In 1901 the plant at Port Richmond burned the garbage at a cost of about one dollar per ton.

Ashes and rubbish are generally used for filling. It is intended in the future to erect furnaces for burning the dry rubbish.

Dead animals are removed under separate contract at $41,000 per annum for Manhattan alone. Street sprinkling is done by private contract with the residents and property owners. Dr. Soper estimates the value of the different classes of New York refuse when separated, collected and delivered at the disposal works to be: For garbage, $2.47 per ton; for street sweepings, 5 to 13 cents per ton; for paper and wood refuse, $4.80 per ton.

He does not think that the outlook for a solution of the problem of final disposal very encouraging at present, and indicates the lines of investigation which he believes promise to lead to desirable results.

Prof. Olin H. Landreth has contributed a paper on the disposal of refuse in the city of Buffalo. It is chiefly interesting and valuable on account of the practical data and items of cost contained therein.

The refuse is divided, first, into ashes and all other dry refuse, both combustible and non-combustible; second, garbage proper; and third, dead animals, each of which is collected separately. Ashes and dry refuse are used to fill old lots. At present a study is being made as to an improved system of disposal by utilizing combustible matter at a proposed sewage pumping station, and of sorting out the marketable material. A study is also being made into the feasibility of trolley transportation at night from sub-stations, where the rubbish is collected during the day, to the main stations for final disposition.

The garbage is collected by the city and reduced by the Merz extraction process just outside the city limits. The city pays $15,840 per annum for the latter until June, 1903. No objectionable odors are said to arise from the plant. The daily amount of garbage handled was fifty tons in February, to 140 tons in September, the annual average being 85 tons per day.

To operate the works there is daily required 14 tons of coal, 100,000 gallons of water for steam and condensing purposes, and 50 gallons of naptha to replenish losses.

Between January 1, 1898, and September 1, 1899, the cost of collection and disposal was kept in detail, and from this information Prof. Landreth finds the cost of collection and dumping ashes and dry refuse per cubic yard to have been 37 cents; of collecting and delivering garbage to the disposal plant per ton, $1.87, and of reducing the garbage at the plant, 62 cents per ton.

When reduced to per capita rates the total cost of collecting and disposing of all sorts of refuse during recent years was materially less than before, due to the change from a lump sum payment per month to a unit rate payment. So far as a comparison can be made it also appears that of late years city collection is more expensive than a collection by contract. The reduction of the garbage is deemed satisfactory, and studies are now being made to ascertain better methods of collection of the refuse and of the disposal of the ashes and dry refuse.

Dr. Byron Stanton has presented a paper on the disposal of refuse material in Cincinnati from 1872 to the present time. This paper is valuable because it records the history of a branch of public service in Cincinnati which has been much neglected, and constitutes a series of unsatisfactory solutions of this problem which are useful in guarding others against duplicating the experience. The refuse is there divided into ashes and dry rubbish, both of which are disposed of by dumping, and into vegetable garbage, animal garbage, slaughter house offal and dead animals.

In 1872 a ten-year contract was made for the disposal of waste, except the ashes and dry refuse, for $15,000 per annum. Subsequently another ten-year contract was made with another party for $2500 per annum. Neither contract was satisfactory, and in 1892 it was determined to use a reduction process for extracting grease from the vegetable garbage, another party having the contract for the removal of animal garbage and dead animals. Neither of these contracts gave better results than those previously in force.

In July, 1902, the present contract was entered into by a single party for the removal of vegetable and animal garbage as well as dead animals, for a term ending 1907. The reduction is by the liquid separator process, which has been tried in some other cities, as Detroit and Cleveland. Time only will indicate whether or not this new venture has merits. The city pays for the disposal $76,000 for the year 1903, the same gradually increasing to $80,400 in 1906, which includes the cost of collection three times weekly in summer, twice in winter, and daily from markets, hotels and other places where there is a rapid accumulation of decomposing matter. Water-tight iron carts are hereafter to be used.

Ashes and dry refuse are excluded from this contract, and are taken to dumps or used for filling up to grade streets in the lower parts of the city.

Dr. Charles V. Chapin has presented a paper which is interesting from the fact that the author has made a special study of the question of refuse disposal for many years. His historical review is instructive, showing the line of development and failures in the city of Providence, because after a long experience he returns apparently, to the starting point, but rests on a more rational and scientific basis. The question of ashes and dry refuse is eliminated from his discussion.

Swill, or kitchen garbage, was in former years collected at little or no expense to the city, and was fed to swine. The method was not satisfactory, and since 1874 contracts for the collection and disposal have been in force, starting with a compensation of $6100 per annum and rising to $14,500 within a few years, and during the last year reached the sum of $27,500. The author urges daily collection instead of twice or three times a week, and believes that by far the most urgent improvement in the management of the garbage business is daily collection in hot weather. He advises metal wagons for this purpose, and finds them cheaper than wooden wagons, and also that large ones holding about 96 cubic feet, even when requiring three horses, are more economical than smaller ones.

From 1890 to 1893 the garbage was disposed of by the Simonin Reduction Company, described by the author at the Mexico meeting of this Association. By this process the grease was extracted by the naptha method. It promised success, but was a failure, because it was not remunerative to the contractor.

After its abandonment the garbage was again fed to swine, and continues to be so disposed of to the present day. The contractor collecting the garbage sells it to farmers for about 25 cents per cubic yard. It is consumed so quickly and thoroughly that very little odor arises, the feeding grounds being away from roads and dwellings.

The disposal, irrespective of collection, has never cost the city one cent, and at times has even lessened the cost of the collection, which the author gives for New England as being 10 to 29 cents per capita. When garbage is fed to swine the cost to the city has been reduced below the cost to the contractor for collection. It ranges from 2 cents per capita at Holyoke to 10 cents at Portland, and in several cities, such as Lynn and Cambridge, even a considerable revenue is derived.

In speaking of this method as the only one by which garbage can be disposed of without cost, Dr. Chapin believes that under certain regulations it is the preferable method for many cities.

As to the objections that have been urged against it, he mentions two: That it creates a nuisance and that it is dangerous to health. The nuisance, he thinks, is no greater than that of any other disposal, and it can be done away with as well as the nuisances associated with reduction or cremation. The danger to health, he believes, is practically confined to trichinosis, and believes that the rarity of the disease and the ease with which it can be avoided removes this danger from consideration. The supposed poor quality of pork resulting from garbage feeding can, he thinks, be avoided by feeding fresh garbage through a daily collection and by better methods of feeding.

Finally, he believes that the question of garbage disposal is not one of health, but of comfort, and that garbage removal is a work not for the Department of Health, but for the Department of Public Works. It means a disposal with the least possible nuisance and the least possible cost.

Dr. Hibbert Winslow Hill, who contributed the description of the Boston works last year, reports the progress since made. The removal of the garbage reduction plant from the city to Spectacle Island is under way, and the new buildings are practically completed. It is proposed to move one-half of the apparatus at a time, running the remaining half double time. Complaints with regard to odors were less frequent during the year, because it is stated that all possible means were adopted to mitigate them. As the new site lies eastward of the city, and as the prevailing winds are from the east, Dr. Hill states that the difficulty may possibly not be entirely obviated by the removal.

From other cities where likewise interesting experiences are being gathered, additional papers have been promised for next year.

In England, a disposal of the entire city refuse by cremation is still the sole method advocated and practiced there. In France, Belgium and Holland the subject is now under discussion, the respective works not being satisfactory. In Hamburg, which is still the leading city in magnitude and operation of works for refuse destruction by cremation, the reports continue to be satisfactory as regards both efficiency and cost. In Berlin, destruction by fire was found quite expensive because during a large portion of the year it was necessary to add fuel. The utilization of some parts of the garbage has there been advocated, but not yet finally adopted. The extraction of grease is not considered because of its small quantity. The production of heat even for garbage destruction is not found to be economical. The manufacture of saleable articles has been considered, discussed and tried, but as yet without a promise of permanent success. The latest suggestions emanating from professional sources are separation of the refuse into the same three classes into which we generally divide the material in this country, namely, ashes, dry refuse and kitchen garbage, and to dispose of them separately. The ashes are to be used for filling, the dry refuse is to be burned and the kitchen refuse is to be hauled to neighboring farms and dug into the ground for gradual conversion into fertilized soil.

From what is said above, the present state of the art of disposal of city refuse has not advanced materially in the last year. Many of the largest cities in America have reduction plants for the extraction of the grease with more or less success. A large number of cities, and particularly those with a small population, cremate their garbage, mostly with little success and few with apparently satisfactory results. The reason for this fact has been pointed out repeatedly in previous reports.

The latest, and in some parts of the country still the most common, method of disposal, namely, that of feeding kitchen garbage to swine is now, as already mentioned, earnestly advocated by a prominent health officer, although most ardently opposed by most sanitarians for many years. It is advocated because under many conditions it is not unhealthful if properly conducted, and it is more economical in smaller communities than any other method.

The Berlin proposition of converting kitchen garbage into compost has not yet been sufficiently tried. The results with this method in this country do not seem promising, except where the soil is quite barren and the cost of labor very low.

The practical solution for any city, therefore, seems to lie in a study of all of the appertaining local conditions and in the selection of whichever of the above methods may be found most economical under the existing local circumstances.

REFUSE DISPOSAL IN MONTREAL.*

By Dr. ELZEAR PELLETIER, Montreal, P. Q.

I have the honor of submitting to you the following report concerning the practice followed at Montreal for the removal of household garbage and refuse and the final disposal thereof, the removal of dead animals, the cleaning of privy pits, the removal of manure and the street cleaning:

A few statistics relating to the territory of the city of Montreal will help you to better understand the importance of the different municipal services of which mention is made in this report: Area of the city, 7064 acres, of which 484 form the Mount Royal Park. The city is somewhat oval in shape. Its larger diameter (southwest to northeast) is 5.1 miles, and its smaller diameter (southeast to northwest) is 3.8 miles. The population of the city is 267,516. Buildings (*a*): Private dwellings, 48,654; stores, shops, business offices, 9641; hotels and restaurants, 394. Horses (*b*), 7797; cows (*c*), 655. The collective length of the streets is 180 miles, of which 28 miles are paved and 152 miles macadamized. Of the 28 miles of paved streets, 14 are paved with asphalt, 8 are paved with granite or other stone blocks and 6 are paved with wood. Highest temperature in summer, 93° F. in the shade. Lowest temperature in winter, —25° F.

I. HOUSEHOLD REFUSE.

Definition.—It is important to define just what one means in Montreal by the words "houseshold refuse." Contrary to the practice in a large number of cities in the American Union and in Europe, we understand here by this term everything which families wish to get rid of: (*a*) *Kitchen waste* (called in England, garbage, and in some parts of the United States, swill); (*b*) *rubbish of all sort*; broken crockery, bottles, tin cans, waste paper, rags, old boots, straw, broken furniture, etc. (called refuse in England), and lastly (*c*) *ashes*, which are not here collected separately from all other waste as is usually done in the cities of the United States. Very often, in England, the word "refuse" also comprises ashes.

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* A report submitted to the Board of Health of the Province of Quebec.

(a) The figures given are those of 1898. Since that time, no further reckoning has apparently been made.

(b) We are speaking here only of horses whose owners have taken out business license. No account is kept of "pleasure horses," except for the water tax. No account of their number has been kept.

(c) This figure is given by the Municipal Milk Inspection Department.

ANALYSIS OF "HOUSEHOLD REFUSE" OF MONTREAL.—These analyses are of great importance in relation to incineration. No exact analysis of household refuse has been made. Mr. Doré, municipal sanitary engineer[1] has given me the following approximative composition:

	In summer.	In winter.
Kitchen wastes	65	25
Paper	15	10
Tin cans, bottles, old boots, rags, etc.	10	5
Ashes	10	60
	100	100

For the purpose of comparison, it will be interesting to quote here analyses which Mr. Rudolph Hering[2] furnishes of the garbage and refuse (ashes excluded) of two European and three cities of the American Union:

	London	Berlin	New York	Boston	Trenton, N. J.
Kitchen refuse (animal and vegetable matters)	77	82	64	84	84
Paper	23	11	28	16	12
Rags, clothes, carpets, etc.		3	8		3
Boxes, barrels and other waste		4			1
	100	100	100	100	100

From the above it follows that, *abstraction made of the ashes*, our household refuse differs from that of Trenton as follows:

	Trenton, N. J.	Montreal.	
		Summer.	Winter.
Kitchen refuse	84	73	63
Paper	12	16	25
Other wastes	4	11	12
	100	100	100

COLLECTION OF HOUSEHOLD REFUSE IN DWELLINGS.—By-law 143 of the city of Montreal (passed in 1885) allows, for this collection, the use of *any* receptacle, not to exceed a flour barrel in capacity and enacts that these receptacles be placed on the sidewalks or in the private lanes in rear of the houses.[3]

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[1] Mr. Doré had control of the removal and disposal of household refuse during several years, this municipal work having formed, from 1893 to 1900, part of the duties of the Municipal Board of Health.

[2] Engineering News, September 11, 1902.

[3] These private lanes are numerous in the residential districts.

By-law 256, passed in October, 1900, forbids under penalty of a fine of $40, the mixing of ashes with other garbage and refuse, but the authorities do not insist upon its execution. Keeping ashes separately has no importance as far as the Eastern and Central garbage districts are concerned since all garbage and wastes are taken to dumps as well as the ashes. As for the Western District, in summer the ashes mixed with other waste matters is not in sufficient quantity to interfere with the subsequent incineration of the garbage; and for the winter season the department is presently setting up an "ash separator," which, if it be as successful as the superintendent expects, will make it unnecessary to enforce, even during that season, the by-law in question.

The receptacles are only supposed to be put out on the street or in the lanes a short time before the scavenging carts pass by, yet it is only too notorious that they remain there for a much longer time. There is scarcely a lane where one may not see some of these receptacles, whether full or empty. The subject of receptacles will be again taken up further on.

Removal of Household Refuse.—The removal as well as the final disposal of household refuse devolves, since February, 1900, upon a committee or special department composed of seven aldermen, with a superintendent or executive officer (Mr. Leguerrier) and a secretary-accountant. The committee appoints the foremen, but the superintendent controls the choice of the other employés and their dismissal. The expenses of the department, for the year 1901 have been $64,819.00, of which $12,775.00 went for incineration.

For administrative purposes, the city is divided into three districts: The East District, comprising all the part of the city east of St. Denis street; the Center District bounded on the east by St. Denis street and on the west by McGill, Beaver Hall Hill and University streets; the West District, including all the remainder of the city. Each district is subdivided into three sections, so that each part of the city may be attended to twice a week. The refuse of the West District only is incinerated. The refuse of the Center and East Districts is carted to dumps.

The staff of each district is made up as follows:

West District	For the removal of garbage	1 Foreman. 24 Carters. 24 Single carts. 2 Stablemen.
	For the incineration	1 Foreman, 12 Furnacemen. 2 Carters to remove the ashes.
		1 Night watchman.
Center District		1 Foreman. 25 Carters and helpers (for loading the carts). 2 Men for the dumps. 9 Double wagons. 7 Single carts.
Stable for both the Centre and East Districts		2 Stablemen. 1 Night watchman.
East District		1 Foreman. 1 Dump man. 14 Carters and helpers (for loading). 5 Double wagons. 4 Single carts.

The department owns sixty-seven regular and fifteen supplementary or reserve horses.

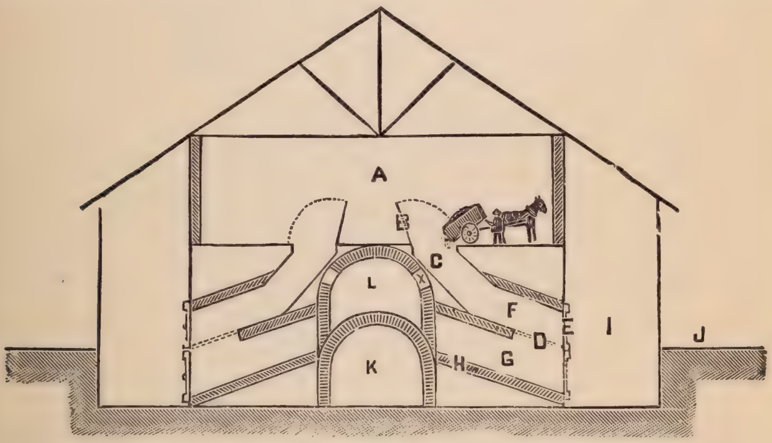
All the wagons of the department are of wood. The single wagons are ordinary dumping carts with a capacity of 57.14 cubic feet (2.10 cubic yards). Three hundred and forty-five cart loads weighed at different dates in September (11, 15, 23 and 24, Sept.), gave an average of 1566 pounds a load. I am told that by the first November, each load averages no less than one ton. The double wagons slope towards the rear end to render unloading more easy, and have a capacity of 163.76 cubic feet (6.06 cubic yards) of refuse. There is a trap in the bottom of the double wagons, but its tightness is not absolutely beyond suspicion. Each wagon is provided with a canvas cover supposed to efficiently cover its contents during transportation. The wagons are not cleaned after use. The scavenging has been done during the day time for over a year, with the exception of the refuse from the markets, which is removed during evenings.

In spring, after the snow has entirely disappeared, the department begins the clearing or cleaning of the private lanes. This cleaning is not performed by the usual permanent staff of employés of the department, and costs from $3000 to $4000.

AMOUNT OF REFUSE REMOVED SINCE THE WORK IS DONE BY MUNICIPAL EMPLOYES.—The municipal sanitary engineer (Mr. Doré) gives the following figures:

Year.	Number of loads (a) (single loads).	Equivalent in tons.
1893	78,067	
1894	78,340	
1895	80,249	
1896	84,541	
1897	83,160	
1898	91,338	
1899	82,269	
1900	81,859	
1901	85,643 (b)	67,058

DUMPS.—Refuse from the Center and East Districts of the city is taken to dumping grounds, the principal one of which lies near Laurier avenue, in St. Denis ward. The refuse is made use of to fill up immense disused quarries. The other dumping ground, on Frontenac street, Hochelaga ward, is an old ravine which at the present moment is almost entirely filled up. From these dumps to the farthest parts of the east and center districts, the carts have to travel from three and a half to four miles. Efforts are being made to cover over with earth the refuse carted to the dumps and for this purpose earth from the



THACKERAY INCINERATOR—Cross section, from a rough sketch made by Mr. J. E. Doré, C. E.

A.—Where loaded carts arrive. C.—Hopper shown with its cover, B, opened. F.—Furnace. D.—Grate. G.—Ash pit. H.—Brick floor. L.—Collective smoke flue. X.—Exit for the smoke from the furnace into the collective flue. K.—Second smoke flue (not utilized). E.—Front (iron) of furnaces. I.—Where the stokers are stationed. J.—Level of the soil.

(a) Including kitchen refuse, other house refuse and ashes.

(b) This last number was furnished by Mr. Leguerrier, the present Superintendent.

various excavations made in the city is invited. The quantity of earth brought in this way is not sufficient, however, for a large part of the refuse deposited since a long time, apparently, still remains uncovered

INCINERATION.—The household refuse of the west district is carried to the Thackeray incinerator, built on the Gregory farm in St. Gabriel ward. The longest haul for the carts is about two and a half miles. All the refuse is at present put at once into the furnaces without any previous separation of the ashes. However, the city is now having installed a "centrifugal separator" for the ashes, which will be in operation during the winter, since at this time of the year the ashes make up more than one-half (about 60 per cent.) of the refuse brought to the incinerator.

The Thackeray incinerator was built in 1894 at a cost of $41,000. It is a crematory furnace of European type (sloping garbage chute and grate) very similar to Fryer's destructor.[1]

The Thackeray incinerator, leaving what appliances are not now or have never been used, whether because they were found useless in practice or for any other reason, is made up essentially of a double battery of brick cells placed back to back, but on each side of a common flue, which in reality is nothing else than the chimney prolonged horizontally to the extreme opposite end of the building. The cells, to the number of twelve (six in each battery) are uniform in size, about twelve feet long and six feet wide. The upper part of the incinerator forms a platform with a hopper above each cell to allow the scavenger carts to dump their loads directly into the cells. The chimney is 175 feet high; its inside diameter is seven feet. A vertical grating at the bottom of the chimney stops all papers which might be carried off by the draught before being burned up.

Since the incinerator was built (*a*) *steam jets* and (*b*) *mechanical fans* for forced draught have been done away with as well as (*c*) *a boiler* placed in the flue to utilize the heat (the chimney's draught has been considerably improved since the removal of this boiler). Moreover, there are no longer in use now (*d*) *a second horizontal flue* immediately underneath the one at present sufficient for the working of the incinerator, and which was intended to be connected at will with the latter in such a way as to make, of the two, one longer flue bent on itself; (*e*) *fourteen small supplementary cells* distributed between the twelve principal ones already mentioned. Openings had been provided for the escape of the heat and fumes of the small cells into the adjacent big cells;[1] (*f*) and lastly, at the base of the chimney a *fume cremator*.

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[1] In the furnaces of American type, the grate is horizontal and there is no sloping chute for drying the garbage previous to its being consumed.

The household refuse of Montreal is auto-combustible during summer, when the amount of ashes mixed with it is about 10 per cent. to 15 per cent. of the total quantity. All is consumed without any previous sifting and without the addition of any fuel. I have found this to be the case from three visits which I made to the Gregory farm during this month of October (1902). This incineration of household refuse without the addition of fuel is to be noted, since most of the incinerators on the continent of America have not given similar results, a circumstance which has given rise to the often stated opinion that such auto-combustion was impossible even when the furnace was fed only with refuse unmixed with ashes. The reason given to back this belief is the quantity of moisture contained in the garbage and refuse and which has been estimated at 80 per cent. in certain cases. Without pretending that the Montreal garbage and refuse contains as much moisture (Mr. Doré estimates the moisture of Montreal garbage and refuse to be 60 per cent.), the experience furnished by the Thackeray incinerator seems to confirm the opinion of Mr. Hering, of New York, that the sloping chute of the furnaces of European type is of great importance by its favoring the evaporation of the moisture of the refuse before it reaches the fire.

The working hours at the incinerator are from 7 a. m. to 6 p. m. The final loading for the day is then done, and is slowly burned during the night. Of the twelve men necessary for the handling of the cells, four are on the platform to load the cells, eight others look after the fires, accelerate the combustion and remove from the cells the ashes and clinkers as they are produced. In the morning all the refuse of the previous day is completely consumed; the furnace is then completely emptied of the still glowing clinkers, less what of them is necessary (one or two) to set fire to the first load of refuse of the day. Thus, the fires once lighted never die out. There is a quick combustion during the day and a slow combustion at night. The fire keeps from Saturday to Monday owing to occasional manipulations, which the watchman undertakes, but as the cells have cooled down considerably the combustion is less active on Mondays than on other week days. The ashes and clinkers which are not used up for road making are carted to the dumping grounds by two carters belonging to the incinerator staff.

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[1] The inventor of the incinerator had thought that these smaller cells would eventually prove useful for the incineration of certain substances which might require the addition of fuel for their destruction, besides their being useful to increase the draft and combustion in the big cells.

I did not detect, on any of my visits, any bad smell resulting from the incineration and everyone that I have spoken to about the matter has always answered that they never heard any complaints.

Cost of Incineration.—From the figures furnished me by the municipal department in charge, the quantity of household refuse incinerated during the year 1901 amounted to 13,659 tons (17,445 loads), at a cost of $12,778.00, averaging therefore 93½ cents per ton of refuse incinerated. However, this amount does not give an exact idea of the cost of incineration, either on account of interruptions in the running of the incinerator (repairs or insufficient amount of garbage) the wages of the men continuing to run just the same, or for other reasons. It is now well established that the net cost for the incineration of a ton of garbage is 39 cents. During the twelve weeks ending on 25th October last, Thackeray's incinerator consumed, without any previous sifting of ashes, 4730 tons (6041 loads) of household refuse at the cost of (salaries of the foreman and his fourteen men) $1854.00 for the twelve weeks. It is estimated that the incinerator can consume 100 loads (78 tons) of refuse a day when running actively by day and slowly (without handling) at night, as it is done presently. It is believed that if its fires were forced day and night, it could destroy 150 to 180 loads in twenty-four hours.

OBSERVATIONS.

A. Receptacles.—The city of Montreal does not prescribe any special receptacle for the gathering of refuse. By-law 143 orders solely "that their capacity must not exceed that of a flour barrel." For this reason, many exceedingly defective ones may be seen in the streets and lanes. Anything from a flour barrel to a fruit basket may be noticed. The empty flour barrel which seems to be somewhat recommended by the by-law, presents the following drawbacks: It is not or soon ceases to be tight, it has no cover and, in winter, the diameter of the top of the barrel being less than that of the body, the kitchen refuse freezes in and often can only be removed with considerable difficulty.

The city, if it does not prescribe, at least might recommend and encourage the use of a uniform receptacle increasing in diameter from bottom to top, provided with a cover to prevent the emanation of foul odors and to protect the contents from rain or snow. To have these receptacles uniform the city should furnish them to the citizens strictly at cost. On account of the climate a wooden receptacle seems preferable to those of galvanized iron, whose use is obligatory in many cities of the United States.

Nevertheless, without further delay the city should decree that the receptacles actually in use should never be filled fuller than to within three or four inches of the top, and that at the very least, those placed in the street will have a cover. This would at once clear our streets of this multitude of small pots, tubs, baskets, etc., the overflow of which now litters our thoroughfares.

Shall people be allowed to deposit waste paper in the receptacles for refuse? These waste papers scattered in the receptacles are too often blown about by the wind as the carts are being loaded, and are a great cause of filthiness for our streets and lanes. In some cities of the United States, the removal of paper is entrusted to a special paper service. On certain stated days in each ward of the city a wagon makes its rounds stopping at all houses where a card hanging from one of the windows of the ground floor indicates that there is paper to be taken away. How far a similar service can be organized in Montreal, it is hard for me to say, I but mention it here to have it submitted to the consideration of the municipal department it concerns.

In any case, if waste paper must be allowed to be thrown into the barrels, the manner in which it is to be done, to prevent its scattering about, should be regulated. I have often remarked at the doors of Chinese laundries, on the days fixed for the removal of garbage large quantities of paper carefully tied up in bundles. Could not the municipality force every family to do the same if they wish the municipality to remove the paper. Those objecting to this rule would have only to burn their waste paper.

B. UNSANITARY CONDITION OF THE LANES.—Since fifteen years the level of certain lanes is gradually rising. Some whose level was flush with that of the surrounding streets are now from one to two feet higher, real disguised dumps surrounding the residential districts. It would be desirable that the city take over these lanes (which up to now have been private property) and pave them. This would put an end at once to the successive and permanent accumulations of filth which will almost inevitably go on unless this is done.

The causes of the unsanitary condition of the lanes are multiple. In the first place a good many families, otherwise particular about the cleanliness of their yards, have no concern whatsoever as regards the lane, which, nevertheless, borders their properties. They have no scruples about throwing all kinds of garbage right into them, especially in winter, when they experience some difficulty in opening the gates of the yards. In some cases the garbage is emptied from the second or third floor of a house or shed down a wooden spout, at the bottom of which a receptacle is supposed to have been placed. The second and most common cause is the tolerating of *rag picking* in the lanes. Very often the rag pickers, to help their work, dump the receptacles (which are there waiting for the municipal wagons to pass) and then sneak away leaving the rubbish scattered over the ground. Then along come the scavengers, but they only gather the contents of the receptacle without worrying themselves about the scattered litter. More is not apparently required of them.

In winter, especially in February and March, the lanes often are impassable, and as a result, the service becomes irregular. The consequence is that the receptacles at the family's disposal no longer suffice to hold the refuse of the house. The surplus is thrown into the lanes where it remains until the snow melts away entirely, in May. My observations during many consecutive winters permit me to state that in a great many cases the snow blockades of the lanes is solely due to the shoveling of the snow by the citizens to enable them to open the large gates of their yards which almost invariably open on the lanes. To remedy the situation in these cases is a very simple matter; it would suffice to oblige the proprietors to have their yard gates open on the opposite side to the lanes, or as an alternative to have them cut, in the large gates (about two feet above the ground so as to be above the snow level during most of the winter) an opening large enough for the passage of the receptacle (an expense of half a dollar). By so doing the opening of the large gate during the winter would be dispensed with. To prevent deposits of refuse in the lanes and even close to sidewalks in streets, tenants should be instructed in their duties so as to prevent them pleading ignorance. This might be done by publishing in the three or four most used languages in Montreal the municipal by-laws concerning the gathering of refuse, mentioning the penalties provided for their infraction and distributing a copy of these by-laws in every house.[1]

Rag picking should no longer be tolerated either on the streets or in the lanes. The following by-law would soon stop it, if orders were given to the police to make a few examples of the offenders:

By-law 143, Section 2: No rag gatherer or other person shall overturn or in any way disturb or interfere with the contents of any such box, tub or barrel when placed as aforesaid for removal by the scavengers, nor shall any person break, injure or take away any such box, tub or barrel when emptied, to the detriment of its rightful owner.

Section 4 of the same by-law provides a fine not to exceed $40 or imprisonment not to exceed two months for every infraction of the above regulation.

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[1] It may be remarked by the way that the city of Montreal is too negligent about promulgating its various sanitary regulations. These are often violated by families through ignorance.

Rag picking could only be allowed at the incinerator or at the dumping grounds. I am of opinion that immediate appointment of an "*inspector of lanes*" should be made, whose sole occupation would be to see that the lanes are kept clean both winter and summer. This nomination would put all responsibility on one man, and consequently better supervision than formerly would be kept up. Since the city expends between $3000 to $4000 each spring for the cleaning of the lanes, the appointment of an "inspector of lanes" would not mean additional expenditure, but should, on the contrary, even taking his salary into consideration, result in an economy for the department.

C. REMOVAL OF THE HOUSEHOLD REFUSE.—Much good can be said of this municipal service, but it is still susceptible of improvement.

Should the removal be done by day or by night? During many years the scavenging has been done by night, but on account of numerous complaints, such as forgetting to empty barrels, lack of care, etc., the department has had the work done by day, so that the workmen are in a sense under the supervision of the interested families, a supervision which is well worth many other and which certainly tends to improve the work of the employes.

This day work is open to many objections in the parts of the city where there are no lanes in rear of the dwellings; among others, the business districts. The receptacles then form on each side of the street an almost unbroken chain whose appearance is anything but sightly, not to mention the vile odors they emanate, and this is in permanence in many streets from six to eight hours each gathering day. If all the dwellings opened on lanes, where the receptacles might be placed, I could not, on my part, see any objection to having a day service done there. And if after consideration of the subject, the department would deem it possible to divide its men into a day staff for the lanes and a night staff for the streets[1], which could be probably made possible by a rearrangement of the districts, this method would be perhaps, under the circumstances, the most suitable for the city of Montreal. If, however, the department should believe this simultaneous day and night service impossible, then the only thing to be done would be to have the whole work done at night, as formerly. This presents no difficulty for the streets, which are sufficiently well lighted to have good work easily done by careful employés. As to private lanes, if each wagon had a powerful lantern or a torch, the difficulties met with under the former night system would be considerably lessened.

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[1] Each staff could do night work alternatively for one week.

D. Wagons Used for the Removal of Refuse.—It is advisable to recommend the use of wooden covers which would replace with advantage the cloth covers now in use (often too small and badly fastened to the wagons). The wagons should be washed out with a hose after use during summer, from May to November. This cleansing is obligatory all the year round in several cities of the United States, according to Chapin[1], to do away with any offensive odors.

E. Employes.—The necessarily dusty work performed by the employes either during removal of the garbage or its incineration calls for the use of overalls which should be furnished by the city, as is the practice elsewhere. At New York these overalls constitute a uniform which is not prejudicial to esthetics, gives an official character and inspires the public with a consideration for these municipal servants they are unfortunately presently deprived of here. A wardrobe wherein their garments may be left and a washroom, which they could make use of before returning to their dwellings, should be installed in each district. The hygiene of the laborers demands it.

F. Instructions to Scavengers.—Certain employés take no precaution in handling the receptacles, which results in garbage being scattered on the ground and which they neglect to gather with the shovel. Frequently after the receptacle has been emptied, they whirl it from the top of the wagon to the ground, where the garbage adhering to its sides are strewn about. Lastly, do they meet with garbage evidently scattered before the arrival of the wagons, they often pass them by. All this should be the subject of severe enactments, and their infringements punished. According to Waring[2] all similar negligence on the part of employés is punished in New York; for the first offense the employé loses one day's wages; for the second offense, three days' wages; for the third offense, five days' wages, and finally, for the fourth offense, he is dismissed.

As in New York, the hands entrusted with the removal of the garbage should have the powers of sanitary police in so far as their work is concerned and thus they should be instructed to report to the foreman of the district, any negligence on the part of citizens, such as defective or overfilled receptacles, scattered wastes, etc.

G. Dumps.—It was intended, when the Thackeray incinerator was built in 1894, to erect later on two others for the center and east districts. Unfortunately this project has not matured. It would be advisable for the city to definitely settle this question so that dumping grounds which are always a cause of insalubrity may be done

[1] Chapin, Municipal Sanitation in the United States.

[2] Waring, Street cleaning and the disposal of a city's waste.

away with. The present dumping grounds of the city are no exception to this general rule. Notwithstanding the efforts that evidently are made to cover over the refuse as they are deposited, this is not achieved, and effluvia from the dumps constitute a danger for the neighborhood. It must also be borne in mind that before long these dumping grounds will be turned into buildings lots and immediately built upon, with the known results as regards the hygiene of the families occupying them.

II. REMOVAL OF DEAD ANIMALS.

The removal of dead animals is given out by contract which gives the city a yearly revenue of $489. The contract covers a period of four years. The contractor removes from the markets the debris of animals, calls twice a day at the office of the municipal board of health for any orders for the removal of dead animals, in the streets, lanes and yards; is obliged to receive and dispose of fish and meat in process of decomposition, brought by traders to his establishment. The only interest manifested by the municipal authority in the establishment kept by the contractor is to enact (by-law 254) that it must be located at a distance of at least one mile from the limits of the city.

The contract calls for the use of closed wagons, but a dray is tolerated during summer, and an ordinary sledge in winter for the removal of horses, provided the carcasses are covered with canvas.

Should the contractor neglect to remove an animal, the board of health after having vainly awaited the execution of its order, has the removal performed at the contractor's expense. As a rule, the contractor extracts the grease of three-fourths of the animals or animal debris, which he carts or are brought to this establishment, and only buries that which is decayed and at the same time the refuse from the rendering.

There does not yet exist in Montreal a knackery yard built according to the exigencies of hygiene. The installations which have been so far made outside of the city are not stationary and they have often been displaced through disgust by the municipalities in whose jurisdiction they happened to be.

Our Board has often been asked to look into the nuisance caused by these establishments, and has suggested the construction of a modern knackery establishment such as exist in France, but without any result so far.

III. CLEANING OF PRIVY PITS.

There existed, at the beginning of this year, 4194 privy pits, but owing to a municipal by-law ordering their suppression within a delay now expired, the municipal board of health figures the disappearance of about two thousand before January 1st, next.

The cleaning of privy pits is given out on contract. In 1901 the contractor emptied 829 pits, the contents of which amounted to 60,843 cubic feet. The cleansing and subsequent removal are at the cost of the owner or the lessee, who pays the contractor 6½ cents per cubic foot. For its share, the city pays the contractor $3600.00 a year for the incineration of the contents.

Pneumatic apparatus is not made use of for cleansing pits. Covered barrels, hermetically closed with bolts, are used for the removal; they are not washed after being used.

Cleansing and removal are made by day. The incinerator built by the contractor has not been in operation since a couple of years. The contents of the pits are deposited on farms and covered over, according to the information given me.

IV. MANURE.

The by-laws enacted by the city of Montreal pertaining to manure are the following:

"By-Law 46. Section 6. No person shall throw, cast or lay, or cause to be thrown, cast or laid, any house offal, filth, *manure* or rubbish of any kind whatsoever, in any street, lane or alley, private or public, or any public square, place or vacant lot; except the same be placed there for the purposes of and in the manner provided for in this by-law.

"Section 13. Manure shall be kept in tight boxes in the yard of the premises where such manure is made. Occupants of villa lots in the said city having a superficial area of at least ten thousand feet and part of which is under cultivation, may keep manure for the exclusive use of such lots, provided the same does not inconvenience the neighbors, and subject to such directions and orders as the health officer may give in the matter.

"By-Law 197. Section 1. Every cart, wagon or other vehicle used to transport *manure*, garbage, house offal or other offensive or nauseous substance, in or through any of the streets of this city, shall be fitted with a good and substantial box thereon, the sides of which shall not be less than twenty-four inches high, so that no portion of the contents thereof shall be scattered or thrown into the street; and every such box shall be provided with a proper covering so as to conceal its contents and prevent the escape of offensive exhalations therefrom."

Unless complaints are made, the municipal board of health tolerates manure bins in lanes, as they are thus at a greater distance than if in yards, as required by the above by-laws. As to the bins being water-tight, and the removal of manure in covered wagons, it may be said that the by-law is practically a dead letter; water-tight bins are the exception and farmers cart away the manure in wagons having no cover whatsoever, not even a cloth.

The regulations stipulate nothing as to the frequency of the removal of the manure. Thus it is of common occurrence in summer, just when manure is most offensive, to find large heaps, because the farmers, who are busy in their fields, neglect the emptying of the bins.

It is true that in the absence of more precise regulations, the sanitary inspectors, while on their rounds through the city, order the removal of the manure if it exceeds a cart load, but, as it is difficult to attend to all such accumulations, an order is too often waited upon to empty the manure bins.[1] Moreover, it suffices to notice the capacity of most of these bins, to be convinced that their owners intend them to contain more than a cart load of manure. One of these located in a lane near St. Louis Square, measures exactly 16x6x6 feet, and thus can contain ten cart loads.

How may be prevented too large accumulations of manure? In my opinion from livery stables only, and such others which may be compared to them by the number of horses they shelter, can the city hope to obtain a regular removal of manure at the owner's own cost. Regarding private stables, the nuisance caused by manure, will indefinitely continue until the municipal authorities will have organized a municipal service, which could be given out on contract.

This service, far from increasing the budget of the city, should, on the contrary, be a source of revenue. There are in the city 7797 licensed horses[2] and likely as many called "pleasure horses," forming a total of about 15,000. There are 655 licensed cows and about 150 more kept by families who are not milk dealers, forming a total of 800 cows. Thus assuming that each animal (horse or cow) produces two cart loads of manure a month[3] the 2300 animals in question will during a year produce (24x2300) 55,800 cart loads of manure, the sale of which to the farmers would yield far more than the cost of the removal.

The municipal by-laws concerning manure should be immediately revised in order to facilitate the work of the municipal sanitary inspectors. These new by-laws could, among other measures, enact:

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[1] Sanitary inspectors give this order under authority of a regulation concerning nuisances in general. An accumulation of more than a cart load is considered a nuisance.

[2] That is, horses the owners of which have to take out a business license.

[3] This is the estimate given by the chief sanitary inspector of the municipal board of health.

First, that no manure bin exceeding two cubic yards in capacity (the contents of one cart) will be tolerated, existing boxes to be reduced if required; second, that the bins be made absolutely water-tight, and the present ones, that are not so, be replaced; third, that from livery stables—and any other horse and cow stables containing six or more horses or cows—manure will have to be completely removed every third day, except from the first of November to the first of May, when it can be removed weekly; fourth, that from all other stables, the complete removal of the manure shall be effected at least once a week, unless it be pressed in water-tight barrels or boxes covered so as to prevent all escape of foul odors.

V. STREET CLEANING, WATERING AND SNOW REMOVAL.

I append the summary answers given by the superintendent of the road department to a series of questions:

1. How often are macadamized streets cleaned? Ans. Big cleaning up in the spring, then always cleaning up some street.

2. What force of men and horses employed? Ans. In the spring as many carts as we can get, afterwards thirty-two laborers and sixteen carts

3. How are paved streets kept clean, (*a*) asphalt streets, (*b*) wooden paved streets, (*c*) granite or other stone paved streets? Ans. Plenty of water, horse-sweepers and hand sweepers.

4. How many men and horses employed to do the work on paved streets? Ans. Men, 40; horses, 26; horse sweepers, 12; watering carts, 6.

5. How many dumps and their situation? Ans. Harbor front, 3; quarry Fletcher's field and at Victoria bridge.

6. How much is spent on average on street cleaning a year? Ans. Paved streets about $35,000; unpaved streets about $35,000. (N. B. Collective length of macadamized streets 152 miles; collective length of paved streets 28 miles, of which asphalt 14, wood 6, granite. etc., 8.)

7. How often are streets watered? Ans. Every day, all day in dry weather.

8. Is it by contract? Ans. No.

9. How many men, horses and carts employed. Ans. Ninety carts.

10. What does "watering streets" cost a year? Ans. From $12,000 to $20,000, according to the wetness or dryness of the summer.

11. Does the department undertake all snow removal? Ans. Yes.

12. Where is it dumped? Ans. Fletcher's field, Atwater avenue, river, vacant lots in St. Denis and Hochelaga wards and Park Lafontaine.

13. Cost to the department yearly? Ans. From car track streets from $10,000 to $40,000 a year; for other streets, $7,000 to $12,000.

It is notorious that the streets of Montreal are in an *intolerable* state, though *tolerated*. The paved streets even are not an exception to the rule. The durability of their paving does not compare with that of other cities; whose climate is at least nearly similar to ours. The causes for this difference in Montreal are, probably: First, the quality of the work in certain cases; second, paving often laid down too soon after excavation; third, negligence in not immediately repairing the defective spots; fourth, the facility with which several electric or other companies in turn tear up our streets, a fact not foreseen by the road department.

As to the cleanliness of certain streets, it would certainly be improved if people were kept from throwing refuse, paper and dirty water in them. In this connection, the police department could help the road department in looking after the enforcement of the municipal regulations: No. 39, Sec. 13, and No. 270, Sec. 17.

THE COLLECTION AND DISPOSAL OF GARBAGE IN PROVIDENCE, R. I.

By Dr. CHARLES V. CHAPIN, Health Officer, Providence, R. I.

Garbage, or as it is commonly called in New England, "swill," has in Providence always been collected separately from other forms of refuse. The separation is as complete as in any city with which I am acquainted, and more complete than in most cities outside of New England. When my predecessor was elected to office in 1856 the garbage had for a number of years been collected and removed by a contractor at a cost of from $800 to $1600 per annum, the population of the city being at that time less than 50,000. In 1856 the city was divided into small districts, and the collection of garbage was let to small contractors who agreed to remove it without charge to the city. It has frequently been the experience of cities that by dividing the city into small districts, contractors could be found who would agree to collect and remove the garbage for little or no compensation. Men are willing to do this, because they expect to get their return by feeding swine. They are willing to take the risk of success or failure with a few swine, and usually with them the raising of swine and the collection of garbage is only incidental to other farming operations. As these small contractors do not have much at stake, they are usually careless in their work and not infrequently break their agreements at inconvenient seasons. Usually the city is put to expense in collecting garbage which they fail to remove, and the citizens are put to much annoyance. Of late years Denver has let out its garbage collection to contractors who remove it for nothing, but the collection is not satisfactory. This method did not prove satisfactory in Providence, but it was continued until 1868, when a responsible contractor was found who removed the garbage in a satisfactory manner, without cost to the city, until 1873. The contractor was a large manufacturing concern, and the collection of garbage and feeding of swine was a very minor one of its many lines of business. The concern is said to have lost $50,000 on its venture. In 1874 a five years' contract was made for the collection and disposal of garbage, the compensation ranging from $6,100 to $7,150 per annum. In 1879 a ten years' contract was entered into for $6,600 per annum, but before its termination, by mutual agreement, the compensation was raised to $14,500. Then followed two five year contracts, at a rate of 15½ cents per capita per annum, and since then the same contractor has carried on the work, at the same rate, under annual agreements. Last year the population was estimated as 178,000, and the cost of the collection and disposal of the garbage was $27,590.

The garbage has always been collected twice a week in winter, and three times a week in summer, and for fifty years the health officials have vainly urged that it be collected daily in summer. Tri-weekly collections are very general throughout the United States, and the writer is convinced that by far the most urgent improvement in the management of garbage business is daily collection during the hot weather. The garbage is collected in covered wooden or metal wagons. The contractor has found that metal wagons are cheaper in the end. He also, from time to time, increased the size of the wagons, believing that with large wagons the cost of collection is reduced. A number of the large wagons hold about 96 cubic feet each, and some of them are drawn by three horses.

With the exception of four years, 1890 to 1893, Providence garbage has always been fed to swine. During those four years the garbage was disposed of by the Simonin process, which was described by the writer in a paper read before this Association at its Mexico meeting. By this process the grease was extracted and the garbage dried at the same time, by treating it with hot naphtha. It promised success, but was a failure. The reduction could not be done with any profit, though I believe it might have been done without any nuisance. As a matter of fact a great nuisance was created, because in order to save money the works were operated in an improper manner.

With the exception of the four years just mentioned, the garbage has been removed from the city and fed to swine. Most of the time it has been removed in covered wagons, but for ten years or more it was removed in open dump cars, a method which caused much nuisance along the road. Usually the contractor has removed it in the wagons in which it was collected, but for four or five years nearly half of it has been transferred to farmers' wagons, in the city, which is very objectionable, and the practice will be stopped. The contractor sells it for about 25 cents a cubic yard. On the large farm where most of the garbage of Providence is fed to swine, the land is divided into woodland and open, where the swine roam at will, having plenty of room. The garbage is scattered about on the ground and is consumed so quickly and thoroughly that very little odor arises, and as the feeding grounds are away from roads or dwellings, little nuisance results.

In considering garbage disposal (as distinct from its collection) it is seen that for the last fifty years or more it has never cost the city a cent, but instead has at times been a considerable factor in lessening the cost to the city of the collection. The cost to the city of collection and removal for the past 13 years has been 15½ cents per capita per annum, which I am sure does little, if any, more than pay the contractor for collecting, for I find that in 1901 the cost of collection by the municipality was, in Boston, $0.20 per capita; in Cambridge, $0.29; in Brockton, $0.19; in Lynn, $0.19; in Lawrence, $0.10; in Somerville, $0.20; in Worchester, $0.15. Previous to 1889 the cost to the city of the collection and disposal was considerably less than what it cost the contractors to collect. This is quite commonly true in New England to-day when garbage is fed to swine. Thus in Fitchburg the contract price is $0.09 per capita. In Haverhill it is about $0.07; in Portland, Me., $0.10; in Holyoke, $0,02; in New Haven, $0.05. These are very low figures for New England. Moreover, in Worcester the net cost of garbage collection was reduced by the sale of garbage-fed pork from $0.15 per capita to $0.04, and in Brockton from $0.19 to $0.08. In Lynn, Lowell, Brockton, Somerville, Cambridge and Springfield[1] a considerable revenue is derived from the sale of garbage, and formerly this was the case in Boston.

There is no doubt that the value of the grease and fertilizing ingredients of garbage is from two to three dollars per ton, but unfortunately no one has yet found an economical way to utilize it. The food value of garbage is probably much less, but this value can be utilized. Garbage can be fed to swine at a profit and thus the net cost of its collection can be reduced. Health officers and engineers have, almost without exception, opposed this, the only method by which garbage can be disposed of without cost. The general public is also to a considerable extent prejudiced against feeding garbage to swine, and the writer formerly shared this prejudice, but has been led to modify his views.

Objection is made to the feeding of garbage to swine on two grounds. *First:* It creates a nuisance. It is indeed true that this practice does as a rule create a nuisance, but so does every other method of garbage disposal. The writer has seen crematories and reduction works which were every bit as bad as any hog farm. The only difference is that the reduction works cost money, while the hog farm yields a profit. Some years ago Boston sold a part of its garbage for one year for over $27,000, and doubtless the farmers who used it caused more or less nuisance in their neighborhoods. In 1898 the city contracted for the disposal of the garbage at a cost of $47,500 per annum. The reduction plant, which was situated in Boston, caused a very great nuisance. Thus the change from feeding to reduction in Boston stopped the hog feeding nuisance in the country but substituted a greater nuisance in the city, affecting more people and costing the city $75,000 or so per annum. The most generous hearted will hardly approve such altruism as that. It may be argued that reduction, and still more cremation, can be conducted with little or no nuisance. That is true, but so can the feeding of garbage to swine. Almost no attention has been given to minimizing the nuisance from swine feeding, while much attention has been given to so-called sanitary cremation and reduction. Yet the writer will assert that far less nuisance is caused by the hog farms of Worcester and Providence than by the crematories of Trenton and Montreal, to say nothing of the reduction works of Buffalo, Cleveland and Boston. The report of the Minnesota State Board of Health for 1900-01 gives an instance of garbage feeding without nuisance. There can be no doubt that there are many cities near which there is land availible for raising swine, and where the business can be done with very little or no nuisance and with profit. If attention be given to the transportation and feeding, and the best methods are employed, this can be done. Slipshod methods will result in nuisance and failure. Of course very many cities are so situated that feeding to swine cannot be done, and other and more expensive methods must be adopted.

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[1] In 1901 Lowell sold $2,833.20 worth of garbage; Somerville, $1,100; Brockton, $1,232.84; Lynn, $3,788.82; Cambridge, $8,016.71; Springfield, $1,333.53.

Second: It is claimed that the feeding of swine with garbage is dangerous to health. This claim is urged on various grounds.

A. It is said that the pork is likely to be diseased and the disease be transmitted to human beings. Practically the only disease likely to be transmitted is trichinosis. But this is a rare disease, and can be readily avoided by avoiding raw pork. It is said that garbage fed swine are more likely to be infected with trichinosis than are others, but herds of swine everywhere are likely to have some trichinosis and the danger from garbage fed animals is therefore only one of degree. Considering the rarity of the disease and the ease with which it may be avoided, this supposed danger does not deserve further consideration.

B. The pork is said to be of poor quality and to bring a low price on the market. As a matter of fact garbage fed pork is not as hard as corn fed pork, and often brings a little less in Eastern markets. But no evidence has ever been adduced to show that this pork is in any way unwholesome. It is not unlikely that by better methods of feeding, perhaps by cooking the garbage and skimming the grease, or by getting fresher garbage through daily collection, the pork might be much improved.

C. Probably the chief reason that the feeding of garbage to swine is objected to is that the filth theory of disease continues to exert so much influence. We have been so long told that filth and foul odors are the cause of sickness that it seems to be very hard for the public, and even alleged sanitarians, to give up the idea. Because garbage smells bad and hog pens smell bad, they are supposed to be unwholesome. This is pure fiction. There is no reason whatever to suppose that sickness ever comes from such causes. It makes no difference to the health of the town how its garbage is disposed of, or how it is collected, or in fact whether it is collected at all. It is not a question of health, but one of comfort. Garbage removal is work not for the department of health, but for the department of public works. What is needed is the advice of engineers, not of medical men. Garbage should be collected with the least possible nuisance and disposed of with the least possible nuisance. But it should also be done with some regard for economy. It would, in Providence, cost many thousands of dollars more each year to dispose of garbage in any other way than by feeding to swine, and there is no reason to believe the method would be any more satisfactory to the citizens, and would certainly have no effect upon the public health. It would be worse than folly for that city to spend money for the cremation of garbage, when there is such urgent need of funds to properly care for smallpox, scarlet fever, diphtheria and tuburculosis.

THE DISPOSAL OF REFUSE IN THE CITY OF BUFFALO, NEW YORK.

By OLIN H. LANDRETH, Consulting Engineer New York State Department of Health, Schenectady, N. Y.

The fairly complete system of accounting in the matter of the disposal of city refuse in Buffalo, N. Y., makes the study of the question of efficient removal and disposal an interesting one for that city.

In Buffalo the administration of the disposal of refuse is placed with the Bureau of Streets, a division of the Department of Public Works.

The organization of the Department of Public Works is as follows:

DEPARTMENT OF PUBLIC WORKS—COMMISSIONER OF PUBLIC WORKS.

Bureau of Engineering:	Bureau of Water:	Bureau of Streets:	Bureau of Buildings:
Deputy Eng'r Comm'r.	Deputy Water Comm'r.	Deputy Street Comm'r.	Deputy Building Comm'r.

The functions of the Deputy Street Commissioner are:

(1) The cleaning, sweeping and sprinkling of streets.

(2) The removal and disposal of garbage and refuse.

(3) The protecting of the streets from obstruction and encroachment.

(4) The cleaning and inspecting of sewers.

(5) The licensing of carts, hacks, etc.

(6) The inspection of lamps and lights.

In the collection and disposal of refuse Buffalo recognizes three classes:

(1) Ashes and all other dry refuse, both combustible and non-combustible.

(2) Garbage proper.

(3) Dead animals.

For the purpose of proper administration in the collecting and disposal of refuse the city is divided into eight districts, each having approximately the same population.

The subject will be considered somewhat in detail under the heads of "Collection" and "Disposal."

COLLECTION.

Class (1) *Ashes and Dry Refuse*: (2) *Garbage.*—Householders are required to keep ashes and all other forms of dry refuse in receptacles separate from garbage. These receptacles, which are not stipulated in form or size, are brought out on the sidewalk just in advance of the passage of the collecting wagons by men employed by the Bureau of Streets. The collections are made by men employed directly by the city for six of the eight districts, using wagons owned by the city and teams hired by the day for the purpose. For several years past and until August 17, 1901, the collections of these two classes of refuse have been made by contract either at stated prices per cubic yard of dry refuse and per ton of garbage, or by a lump sum per month for both classes. During the summer of 1901, however, the service on six of the eight districts became so unsatisfactory that it became necessary to cancel the contract, which was done. The city seized the wagons, but not the teams, belonging to the contractor as indemnity for the breach of contract and has since August 17, 1901, made all collections of these two classes of refuse in the six districts mentioned. The remaining two districts, No. 1 and No. 8, are still operated by contract by another contractor.

For the work of collecting and delivering the two classes of refuse from the six districts, the city hires continually for six days of each week, forty-five teams. Forty-two of these use city wagons, of which twenty-five are large Watson wagons for ashes, and seventeen are Blake garbage wagons, iron-lined and arranged for rear dumping. The city also uses three hired wagons. Collections, both of ashes and of garbage are made in district No. 1, the central business part of the city, during every night throughout the year. In the other districts collections are regularly made twice weekly from May 1 to November 1, and once a week from November 1 to May 1.

Inspection of the work of collection is secured by a foreman and an assistant foreman for each of the eight districts. As will be seen from Table 2, the average size of loads of refuse under the present arrangement, is about seven cubic yards of ashes and dry refuse per load and three tons of garbage per load. An examination of this table will show, however, that these sizes are by no means uniform or persistent, but that during the past four years the sizes of loads as well as the number of loads per annum has been very intimately affected by the nature of the contract for collection. All wagons, both those for ashes and for garbage are covered, as soon as the loads are made up, with canvas covers. The prices paid for the labor of collection are as follows:

Team and wagon, per day	$4 00
Team without wagon	3 50
Foreman, per day	3 00
Assistant foreman	2 00
Garbage weighers at disposal plant	2 00
Laborers	1 50

All labor and service is based on an eight-hour day, in accordance with the labor laws affecting labor on public works in this state.

In addition to the collection by the city in the six districts, districts No. 1 and No. 8 are collected by contract at an annual price of $21,804. There is also a special collection daily from the markets of the city for which the city pays a contractor $1500 per annum.

The above system of collection covers the entire city with the exception that five of the hotels have private contracts for the removal of their garbage, and a number of the manufacuring esablishments dispose of their ashes and cinders in other ways. There is no city ordinance compelling individuals to deliver their refuse to the city collectors, but the ordinances against the maintaining of nuisances on private premises are fairly well enforced.

(3) *Dead Animals.*—Dead animals are collected and removed to private rendering works, under a separate contract.

FINAL DISPOSAL.

(1) *Ashes and Dry Refuse.*—Ashes and dry refuse are at present hauled out into the suburbs and unbuilt portions of the city and used in filling low lots. The city receives or pays nothing for the privilege of dumping on vacant lots, but consent is readily obtained from owners who are glad of the opportunity to have their property improved by the raising. Although much of the combustible refuse is picked out and carried away for fuel, a considerable amount remains and frequently catches fire, causing a nuisance in the community.

Colonel Frederick G. Ward, the present Commissioner of Public Works, is now considering in conjunction with Mr. C. M. Morse, the Deputy Engineer Commissioner, the subject of an improved system of disposal for this class of refuse. The inquiry, as yet not well advanced, is being directed along the line of a utilization of the combustible matter for fuel at a proposed sewage pumping station, with a possible ultimate sorting out of the more marketable classes of both combustible and non-combustible material to be sold as low-grade junk, the unmarketable residue to be used as fuel for pumping sewage and for filling. This inquiry also includes the question as to feasibility of establishing a number of sub-stations for the delivery of this class of refuse from the wagons used in collection, and the transportation of the refuse from such sub-stations to the general disposal station by special cars operated over the trolley tracks by night. This line of investigation would seem to offer promise of improvements in both effectiveness and economy of collection and disposal, and the results of the inquiry in this case may well be watched with interest.

It is hardly necessary to say that in the design and operation of the scheme the requisites of public sanitation will demand careful consideration, not only in the work of collecting, but also in the transfer, transportation, sorting and final disposal or distribution of the resulting materials.

(2) *Garbage.*—All of the garbage collected by the city is reduced by the Merz extraction process operated by the Baynes Garbage Reduction Company, whose plant is located just outside the city limits in the town of Cheektowaga and about four miles from the business center of the city. The city of Buffalo delivers the garbage at the plant of the company and pays $15,840 per annum for the reduction under a five-years' contract, which will expire on June 19, 1903. During the early part of the contract period, much trouble was experienced from nuisances created by the company in the reduction, which was at that time effected by the use of steaming in digesters and subsequent pressing and drying. On October 1, 1900, the plant of the company was destroyed by fire and after some delay the company reconstructed the plant on the present system, which, so far as I have been able to ascertain from repeated inquiries, has given no ground for complaint as a source of objectionable odors.

The process in outline is as follows: (1) The garbage is charged from an upper floor into revolving dryers each composed of a horizontal steel cylinder rotating on a central horizontal shaft driven by an outside bevel gear; the cylinders are without heads, but the revolving shell fits closely against fixed heads through which the charging and discharging chutes pass and through which the products of combustion from the dryer-furnaces enter and leave the dryer. The drying is accomplished by the heat from anthracite coal furnaces, of which one is placed beneath each dryer; the shell is protected from the direct radiation from the incandescent fire by a brick arch thrown over the grate, but the hot gases pass up and around the shell and thence enter the dryer through the rear fixed head and after passing to the front through the dryer pass out through the front fixed head. The temperature inside the dryer is not allowed to go above 400 or 500 degrees, Fahr. This comparatively low temperature is possible owing to the cooling influence of the evaporation of the moisture from the drying garbage. When the garbage reaches such a state of dryness that there is not sufficient evaporation of moisture to hold the temperature down to this limit in temperature, the operation is suspended, as a higher temperature would tend to carbonize a portion of the grease and interfere with a successful extraction. When this stage is reached the partially dryed garbage has about the consistency of very stiff mortar, and carries about 25 per cent. or 30 per cent. of moisture still. (2) The garbage is now forced out of the rear discharging door of the dryer by the addition of green garbage at the front end, and is taken by a conveyor over to the extraction tanks, where it is submerged with naptha to extract the grease. The extraction tanks are four in number, each ten feet high by seven feet in diameter and hold about five tons of partially dryed garbage each. From 1600 to 1800 gallons of naptha are required to submerge a tank of garbage. The extraction is carried on at a low temperature. The naptha carrying the grease in solution is drawn off and the naptha separated from the grease by distillation and subsequent condensation of the naptha. The grease reported by the manager of the company amounts to about 2 per cent. of the weight of green garbage. The loss of naptha from leakage and incomplete separation from the garbage and grease amounts to about 1400 gallons per month. (3) After the extraction of the grease the garbage is returned to another set of dryers identical with the first except that the temperature is now allowed to run much higher and practically all of the water is driven off from the garbage. In this form the dried garbage amounts to about 17 per cent. of the green garbage and is sold as a fertilizer base. The combustion gases mixed with the steam from the preliminary dryers which carry the odors of the steaming garbage are led through a spray of cold water and pass thence to the chimney, where they are discharged into the air at an elevation of about 100 feet above the ground.

The amount of garbage handled varies from 50 tons per day in February to 140 tons in September, and averages for the year about 85 tons per day. To reduce this amount of garbage the plant consumes about ten tons of coal per day for the steam boilers which furnish steam for the power to operate the plant, and for the heating and evaporating of the naptha, and four tons of coal per day to heat the rotary dryers, also 100,000 gallons of water per day for steam and condensing purposes, and about 50 gallons of naptha per day to replenish losses.

FINANCIAL CONSIDERATIONS.

An examination of Tables 1 and 2 hereto appended will show that the city of Buffalo during the past four years collected and delivered to the reduction works an average of 25,600 tons of garbage per annum, and has collected and disposed of an average of 314 cubic yards of ashes and dry refuse per annum. Unfortunately for a complete study of costs, the nature of the contracts permit of separating the cost of collecting ashes and dry refuse from that of garbage *only* from January 1, 1898, to September 1, 1899. Since the latter date the cost of collecting and delivering the two kinds of refuse has been combined into one item.

From January 1, 1893, to September 1, 1899, however, the separate costs were as follows:

Cost of collecting and dumping ashes and dry refuse, per cu. yd........	$0 37
Cost of collecting and dumping ashes and dry refuse per capita, annum.	32
Cost of collecting and delivering garbage to plant, per ton............	1 87
Cost of collecting and delivering garbage to plant, per capita, annum..	15
Cost of reduction of garbage at plant per ton (by contract)............	62
Cost of reduction of garbage at plant per capita per annum.............	$0.045
Total cost of collecting, delivering and disposing of all ashes, dry refuse and garbage per capita per annum, 1898 to 1902....................	$0.410
Total cost ditto for period January 1, 1898, to September 1, 1899........	0.515
Total cost ditto for 1900 and 1901, per capita per annum................	0.328

A glance at Table 2 shows that the time when this decided improvement in total cost per capita per annum occurred is coincident with the time when the style of contract for collection of refuse was changed from a price per cubic yard and per ton, to a lump sum per month for removing all refuse. While this change had the effect of causing the amount of garbage per capita per annum to be decreased, it has had no such effect on the amount of ashes and dry refuse per capita per annum, but rather has tended to increase the amount.

The period of time during which the city has been collecting its own ashes and garbage is rather too short on which to frame a conclusion as to the relative economy of the two systems of collection. So far as it has thus gone, the comparison is rather unfavorable to city collection as compared with the collection by contract just preceding the change. The population of the city during each of the several years included in the above periods has been estimated from the several census returns to have been as follows:

In 1898	333,000
In 1899	343,000
In 1900	353,000
In 1901 (Pan-American year)........................	383,000
In 1902	373,000

Consideration of statistic given in the tables indicates that in the matter of reduction of garbage the city has reached a more advanced point as to economy than in the collection of both garbage and ashes and in the disposal of the latter. The action of the Commissioner of Public Works and the Deputy Engineer Commissioner, in taking up the study of a more improved system of collection and disposal of ashes and dry refuse, would seem therefore to be an appropriate and promising measure.

TABLE I.

REFUSE DIPOSAL IN BUFFALO, N. Y.

1	2	3	4	5	6	7	8	9	10	11	12
Years.	Periods.	Kind of Contract.	Ashes and Dry Refuse.			Garbage.			Garbage, Ashes and Dry Refuse.		
			Loads	Cubic Yards.	Cost.	Loads	Tons	Cost	Special Collections.	Inspection.	Total Cost.
1898	Jan. 1 to Sept. 1..	Per ton and per yard........................	39,635	185.415.2	$62,071.13	4,621	16,775.1	$29,010.13	....	$ 9.564 00	$100.645.26
	Sept. 1 to Dec 31.	" " "	18,947	91,598.0	30.701 55	2,750	10,734.9	18 671.27	....	4.830.00	54.202.82
	Year	" " "	58,582	277,013.2	92,772 68	7,371	27,510.0	47,681.40	....	14,394.00	154,848.08
1899	Jan. 1 to Sept. 1..	Per ton and per yard........................	45,075	219 514 2	73 572.62	4,472	17,361.7	30,048.85	....	9,568 00	113,189 47
	Sept. 1 to Dec 31.	Lump sum per month........................	12,454	91.571.0	....	2,391	9 440 7	....	....	4.784 00	31,772.00
	Year	Mixed	57,529	311,085.2	....	6,863	26,802.4	....	....	14,352 00	144 961.47
1900	Jan. 1 to Sept. 1..	Lump sum per month..................	30,523	212.380.0	....	4,038	13.707 2	....	....	9.598.00	64.574.00
	Sept. 1 to Dec. 31.	" "	12.929	94,961 0	....	2,317	9 174 2	....	....	4,784.00	32.272.00
	Year	" "	43.452	307,341.0	....	6,355	22,881.4	....	....	14,382.00	96,846.00
1901	Jan. 1 to Aug. 17.	Lump sum per month........................	34,274	248,146 0	....	4,581	16,959.2	....	$ 785.94	9.560.00	66,764.58
	Aug. 17 to Dec. 31	6 out of 8 Districts by city teams and men..	14 547	110 668 0	....	2,637	8 484 2	....	888.50	4.784.00	46 899 47
	Year	Mixed	48,821	358,814.0	....	7,218	25,443.4	....	1,674.44	14,344.00	113,664.05
1902	Jan. 1 to July 1...	6 out of 8 Districts by city teams and men..	26,475	184,513 0	....	2,789	8,252.8	....	833.43	7,255.00	65,221.17

TABLE II.

REFUSE DISPOSAL IN BUFFALO, N. Y.

13	14	15	16	17	18	19	20*	21*	22	23	24
Year.	Periods.	Kind of Contract.	Cubic yards of ashes and dry refuse per load.	Tons of garbage per load.	Cubic yards of ashes and dry refuse per capita per annum.	Tons of garbage per capita per annum.	Cost of collecting and dumping ashes and dry refuse per capita per annum.	Cost of collecting and delivering garbage at reduction works per capita per annum.	Cost of treating garbage per capita per annum.	Cost of collecting and delivering ashes, dry refuse and garbage per capita per annum.	Total cost of collecting, delivering and reducing all garbage and refuse per capita per annum.
1898	Jan. 1 to Sept 1..	Per ton and per yard	4.68	3.63	.84	.076	$0.312	$0.141	$0.048	$0.453	$0.501
	Sept. 1 to Dec. 31.	" " "	4.83	3.90	.83	.097	.309	.179	.048	.488	.536
	Year	" " "	4.72	3.73	.83	.083	.311	.154	.048	.465	.513
1899	Jan. 1 to Sept. 1..	Per ton and per yard	4.87	3.88	.96	.076	.350	.142	.047	.495	.542
	Sept. 1 to Dec. 31.	Lump sum per month	7.35	3.95	.80	.082	....	....	.047	.279	.326
	Year	Mixed	5.41	3.91	.91	.078	....	....	.047	.423	.470
1900	Jan. 1 to Sept. 1..	Lump sum per month	7.34	3.39	.90	.051	....	....	.045	.274	.319
	Sept. 1 to Dec. 31.	" "	6.96	3.96	.81	.078	....	....	.045	.273	.318
	Year	" "	7.07	3.60	.870	.065	....	....	.045	.274	.319
1901	Jan. 1 to Aug. 17.	Lump sum per month	7.24	3.70	.97	.066	....	....	.041	.261	.302
	Aug. 17 to Dec. 31	6 out of 8 Districts by city teams and men..	7.61	3.22	.87	.066	....	....	.041	.366	.407
	Year	Mixed	7.35	3.52	.94	.066	....	....	.041	.297	.338
1902	Jan 1 to July 1..	6 out of 8 Districts by city teams and men..	6.96	2.96	.99	.044	....	....	.042	.350	.392

* Column 20 includes three-fourths of the cost of inspection.
" 21 " one-fourth " " "

REPORT UPON THE DISPOSAL OF REFUSE MATERIALS IN CINCINNATI, OHIO, FROM 1872 TO 1902.

BY DR. BYRON STANTON, CINCINNATI, O.

Cincinnati has contributed but little to the solution of the problem of disposal of the putrescible refuse of cities, the systems in vogue here for the last thirty years having been short of the ideal. The defects were partly due to the process of disposal, partly to the fact that the city undertook the collection of the garbage by the force that collected and removed the ashes—a combination that will always lead to unsatisfactory results. But a small part of the garbage will be taken to the sullage boat when combined removal to the dumps is so much easier and less expensive. The cartman is interested in minimizing the labor and the street cleaning department in minimizing the expense.

Some process of reduction by extraction of the grease and conversion of the refuse into fertilizer has been in operation here for the last thirty years. Not much change has been made in the method of disposal in that time except in the process of extraction and of converting the refuse into a salable product.

The manner of collecting the vegetable garbage has been the same until the contract with the company now doing the work was entered into last June. Under the old system the city collected the vegetable garbage and delivered it at the river to the contractor, by whom it was removed to a point on the river bank some miles below the city, where it was disposed of by feeding to hogs or by the extraction of the grease and the conversion of the refuse into fertilizer.

The first contract to which I have access was entered into in June, 1872, with George Thompson, for the removal without the corporate limits, and at least three miles therefrom, of all the animal and vegetable garbage and slaughter house offal, the contractor to provide a wharf-boat at the city wharf and boats for the removal every day of all vegetable offal delivered by the city. The contractor was also "to gather and remove daily all accumulations of animal matter from the dwellings, hotels, boarding houses and slaughter houses, remove promptly the carcasses of all animals dying of disease or killed by accident," all of which was to be done "in such a manner as would as little as possible offend either sight or smell." In consideration of this the city agreed to provide a wharf of which the contractor should have the free use, to collect and deliver to the contractor "all vegetable kitchen offal unmixed with ashes or other refuse, to prohibit all other persons from gathering, hauling or using any animal or vegetable garbage or offal, or rendering, feeding to hogs or cleaning of guts from slaughtered animals, to prevent the rendering of dead animals and decaying and obnoxious animal matter and bone boiling," and to pay to the contractor the sum of $15,000.00 yearly. By the terms of this contract the contractor was to pay to the owners of dead hogs a price per hundred pounds equal to one-third the market price of live hogs of the same class at the time of removing the dead ones; and for cattle and sheep a fair price, according to size and condition, and to pay to hotels, boarding houses and private families a fair value in soap for all grease received.

The parties to the contract agreed to the following explanation of the terms used, viz., "*vegetable garbage* to mean all vegetable kitchen offal unmixed with ashes or other refuse matter; *animal garbage* or *offal* to mean soap-grease from dwellings, boarding houses, hotels and steamboats; *slaughter house offal* to mean guts and plucks of slaughtered animals and tank-stuff; *carcasses* and *dead animals* to mean all animals dying from disease or accident, and not to mean animals slaughtered for human food."

This agreement continued in force for a period of ten years, when a contract was entered into with the Cincinnati Desiccating Company for a like period to perform the same duties with the same privileges, limitations and definitions, but the price to be paid by the city was but $2,500 for each year.

During the continuance of this contract much of the vegetable garbage was disposed of by dumping it into the river until the United States Government prohibited this manner of disposal. The necessity of finding some other means of disposal making the contract no longer profitable, the contracting company soon found a loophole for escape, and the city was obliged to advertise for new bids.

For the disposition of the vegetable garbage, Isaac M. Simonin was the successful bidder, and with him a 10-year contract was made June 1st, 1892, for the erection and maintenance at a point not more than five miles distant from the City Hall of a plant for the "scientific and sanitary disposition of the vegetable garbage in a manner not injurious to health or comfort."

By the terms of this contract the vegetable garbage was to be collected by the city, as before, and delivered at the company's sullage boat at the foot of Main street, but the street cleaning department, that did the hauling, found it much easier to haul the garbage mixed with ashes to the dumps than to haul it a longer distance to the river. The city officials tried to keep up the appearance of decency by the frequent distribution of circulars calling attention to a city ordinance forbidding the mixing of garbage and ashes, but it could not be expected that such a law would be long respected by housekeepers and servants when the cartmen, employes of the city, put the contents of ash barrels and garbage tubs into the same wagon.

In consequence of the manner in which the garbage was collected the Simonin Company, instead of getting from 30,000 to 35,000 tons of garbage in a year, got less than 10,000 tons. The contract proved not to be a profitable one for the company, and complaints of failure on the part of the city to comply with the terms of its contract were frequent, but as vegetable garbage contains but little that is convertible into a salable product, the less it received the better it was for the company.

As the contract with the Simonin Company was for the disposition of vegetable garbage only, a contract was, a little later, entered into between the city and John Jones, assigned to the Jones Fertilizer Company, for the removal of all dead animals and animal matter and offal from the dwellings, boarding houses, hotels, stockyards, slaughter houses, streets, alleys and commons of the city for a period of ten years from July 3, 1893, all collections to be made by the company. The contractor makes no charge to the city for the removal of dead animals and slaughter house offal, nor does he pay anything for the privilege of removing such animals and material.

In 1901, when the contract with the Simonin Company was about to expire, specifications were prepared and bids advertised for the disposition of the vegetable garbage after the expiration of the Simonin contract in June, 1902, and for the disposition of dead animals and slaughter house offal, etc., after the expiration of the contract with John Jones in July, 1903. These bids were to be for a period not longer than five years, as the legislature had limited the time for which a contract could be made by the city to that period. The "cheapest and best" bid was that of Marvin H. Chamberlin and John B. Corliss, of Detroit, and a contract was entered into with them for the removal of vegetable garbage for a period of five years and for the removal of dead animals and animal garbage for a period of three years and eleven months from July 3rd, 1903, when the Jones contract expires. Their method, as stated by them, is the Liquid Separator Process, which is said by them to have been in successful operation in Detroit, Cleveland and Washington for several years. For this service they were to receive in money as follows: For the year 1902 (portion of the year only, and not to include dead animals and offal), $43,000; for 1903 (to include dead animals and offal after July 3rd), $76,000; for 1904, $77,500; for 1905, $78,500; for 1906, $80,400; for 1907, (portion of year only), $35,400. By the terms of their contract collection of all material is to be made by them, from April 1st to October 31st, three times a week, and from November 1st to March 31st, twice a week in the residential and densely populated parts of the city, the territory to be designated by the Board of Public Service. From the markets, hotels, restaurants, fish markets, butcher shops, hospitals, slaughter houses, produce commission houses and all places where animals, game, fish or fowls are killed daily collections are to be made.

It will be seen that the amount paid under this contract is much greater than under any of the previous ones, but the contractors assume the expense of collecting all of the materials to be disposed of, thus taking from the street cleaning department a heavy expense. If the contract is faithfully carried out this will be, I think, a great improvement over old methods as there will be less mixing of garbage with ashes and dry refuse taken to the dumps.

It is to be regretted that the contract had to be made for a period of five years only, as a longer period would have distributed the expense of fitting up the plant over a greater number of years and thus have enabled the contractors to make a more favorable bid.

In place of the old wooden wagons that absorbed moisture and were neither air-tight nor water-tight, the company has provided wagons with water-tight iron bodies so constructed that the bodies can be lifted, when full, by means of cranes from the running gear and deposited upon railroad cars for transportation to the disposal works a few miles down the river. The wagon beds are provided with sheet iron covers that are supposed to prevent the escape of odors, but not being clamped, the constant vibrations of the covers when the wagons are in motion not only permit the escape of foul smells but keep up a most unearthly din, against which the people have protested. So far the efforts to have this abomination removed have been without avail, the promises of the company to abate the nuisance having been made only to be broken.

For the last thirty years all of the Cincinnati plants for the reduction of garbage and offal have been sources of frequent complaint and some litigation on account of the offensive odors. Of the conditions about the new disposal works I cannot now speak, the plant being new and the amount of refuse material disposed of in the hot months being small for the reason that the company was not in position to take up its contract at the time specified—June 1st. The street cleaning department therefore had to do much of the collecting of garbage until late in August, and that collected by the city was mostly taken to the dumps.

Whether the process in use by the new company has merits not possessed by the others that will mitigate the nuisance created by offensive smells, I cannot say at this time. The observations of another year, when both animal and vegetable matter will be disposed of in large quantities, will be necessary to determine what success the company will have in preventing a nuisance in the vicinity of the reduction works.

Disposal by cremation is not entirely free from objection on the same ground, and until it can be made so, disposal by reduction will probably be continued here, where the amount of material to be disposed of is great, for the reason that it is made more economical by the conversion of the refuse into commercial products.

No attention has been paid to the character of utensil in which the garbage is kept by families until removed. Some metallic vessel with a closely fitting cover should be a requirement of a health board regulation.

One thing that could be done to lessen the nuisance caused by disposal of refuse by any method would be to require its more frequent collection.

The night-soil was for many years hauled in the night to a sullage boat, by which it was taken in the early morning to a point below the city and dumped into the river. The U. S. Government a few years ago prohibited that practice, and it is now dumped into a sewer near the river and soon reaches the same destination as before. About 12,000 or 15,000 cartloads were disposed of annually, but the extension of the sewer system and the abandonment of vaults are rapidly reducing the amount of night-soil removed.

The ashes and dry refuse of the city are being used to bring up to grade the streets in low parts of the city, and the valleys of Deer creek and Mill creek will for years to come furnish places of deposit for such materials, which are unobjectionable if kept free from organic matter.

REPORT ON THE DISPOSAL OF THE REFUSE OF NEW YORK CITY.

By Dr. GEORGE A. SOPER, New York, N. Y.

HISTORICAL SYNOPSIS.

Until the year 1881 the streets of New York were cleaned by authority of the police department. The organization of the street cleaning department as a separate branch of the city administration was the result of much agitation on the part of citizens, physicians, and political reform associations.

For the first ten years, the department of street cleaning did little to justify its existence, and by 1891 the operation of the department had become entirely unsatisfactory. In that year Mayor Hugh J. Grant requested a commission of five citizens to investigate the organization and methods of the department with the object of introducing practical reforms.

The report of Mayor Grant's committee was so remarkable for the clearness and exactness with which the needs of the department were defined that it has remained a guide to a proper development of the department to this day. The committee consisted of Morris K. Jessup, C. F. Chandler, Thatcher M. Adams, D. H. King, Jr., and F. V. Green, men of high standing in the community. The practical outcome of the report was the reorganization of the Department of Street Cleaning by Act of Legislature, Chapter 269, Laws of 1892.

Because of the present value of the committee's recommendations no less than for their historical importance a summary of its findings is here given:

It was declared that the department was not efficiently managed as judged by standards of management of well-established private corporations engaged in large enterprises.

First—A source of difficulty lay in the methods of employing labor. The laborers themselves were inefficient and owed their positions to political influence.

Second—The plant of the department was declared to be poorly located.

Third—The plant was considered inefficient.

Fourth—There was a lack of proper organization and disposition of forces.

Fifth—The manner of disposing of refuse was not satisfactory.

Sixth—The law under which the street cleaning department operated was adequate provided it was efficiently administered by a street cleaning commissioner who had the cordial co-operation of the mayor, police justices, department of health, commissioner of public works, and board of estimate and apportionment.

Seventh—The annual appropriation was insufficient.

Eighth—It was pointed out that the police should help to keep the streets clean.

Ninth—That there should be rules for the employment of the men.

It was observed that the management of the street cleaning departent required the same kind of ability as the management of a transportation company or other large corporation employing a considerable number of men.

The report of Mayor Grant's committee is the foundation upon which the present organization of the street cleaning department is based, but the opportunities opened by it for conducting the department in an efficient and economical manner have never been fully realized. At first little was done in accordance with the recommendations of the committee except to increase appropriations. The department remained as it had always been, an organization in which political power was carefully and continuously fostered. It became apparent that there was need of a strong head to develop the possibilities of reform contemplated in the legislative reorganization of 1892. Such a head was found by Mayor Strong when, in 1895, he appointed Col. George E. Waring, Jr., Commissioner of Street Cleaning.

Col. Waring's administration continued for three years and resulted in raising the standard of usefulness of the department to a higher plane than has been known before or since.

It is interesting to observe that Col. Waring's success was achieved along the lines recommended in the report of the committee of citizens in 1891. Prominent reforms were: Business methods of administration, insistence that the laws and ordinances relating to cleanliness and order in the streets be enforced; and improving the methods of collection and final disposition of wastes. Col. Waring's greatest success was undoubtedly due to his power of instilling a spirit of industry among his men. It has been well said that he found the force of sweepers and drivers a laughing-stock and left them a body of honored workmen.

Col. Waring's term of office was too short to enable him to accomplish his whole task. He had first set himself to reorganize the working force of the department, and in this he succeeded. His greater ambition, however, was to devise means for the economical if not profitable final disposition of the city's refuse. Col. Waring's work along this line was in a new direction; he had no other guide than the examples of other cities. He accomplished, at least, a beginning.

Until Col. Waring's time, the ashes, paper, garbage, and other refuse of the city had been collected and carried in a mixed condition to points along the water front where it was picked over by Italians, who had the privilege of extracting bones, bottles, rags, metals, and other salable matters.[1] The residue left after the picking, was taken by scows and dumped at sea or used to fill in low land in the neighborhood of New York. Under Waring's administration the wastes were separated before collection into three classes; garbage, ashes, and paper. Studies were made of the value of the refuse. Garbage was collected from several other cities, and the value of the grease and other constituents of refuse under a variety of conditions was carefully determined. According to Waring's estimates, which are not considered high, average garbage if its ingredients could be utilized, would be worth $2.47 per ton. A practical plant for the separation of the utilizable ingredients of refuse other than garbage was erected and operated for many months. The sale of the useful materials so collected from thirty-one thousand three hundred tons of refuse, netted the city $3,680.

A committee of investigators, not connected with the department of street cleaning, was appointed by Mayor Gilroy in 1894. Mayor Gilroy's committee which investigated the subject of refuse disposal, reported November, 1894, as follows:

First—That dumping the city refuse in the harbor or its contributory waters should be absolutely prohibited.

Second—That the ordinances requiring householders to keep separate the garbage or kitchen refuse from ashes and other house refuse should be rigidly enforced.

Third—That all house refuse should be collected in galvanized iron vessels with tight fitting metal covers of such size that when full they can be handled by one man.

Fourth—That daily collections of garbage should be made by the city and delivered at the dumping wharves into temporary storage or self-propelling boats of some approved type to be furnished by the party having the contract with the city for the final disposition of the garbage.

Fifth—That the garbage should be disposed of by a reduction process, producing fertilizer and commercial grease; and that the city should invite competition by the various companies controlling such systems in order that the greatest benefit to the city may result.

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[1] In 1894 the city derived a profit of about $140.000.00 from this work.

Sixth—That a separate collection should be made of the remainder of the refuse of the city "not otherwise provided for" which should be used for filling Riker's Island or elsewhere, and for transportation of this material a sufficient number of self-propelling boats of approved type should be constructed and owned by the city. It should, however, be provided that whenever the whole or any considerable portion of the street sweepings can be disposed of by contract or sale for fertilizing purposes at a price greater to the city than the value for filling, then such disposition should undoubtedly be made of them.

Col. Waring made an extensive inspection of refuse disposal methods on the continent of Europe in 1896. It was not, however, for him to completely solve the problem of refuse disposal for the metropolis. This problem is not yet solved.

After the end of Waring's administration, the department of street cleaning fell down considerably from the state of efficiency to which it had attained. In 1898, New York City adopted a new charter, adding greatly to its area and increasing its population from 1,957,282 to 3,350,000. Commissioner James McCartney, who followed Waring, was admittedly a political worker, and his influence and that of his successor, Commissioner Percival E. Nagle, reduced the standard set by Waring.

By the consolidation of New York City with Brooklyn, Staten Island, and many large and small communities in the metropolitan district, in 1898, the difficulties of the administration of the department of street cleaning were greatly increased. Practically nothing as yet has been done to bring the problem of a satisfactory disposition of the city's wastes to a solution.

Commissioner Woodbury, a physician, succeeded Commissioner Nagle on the election of reform mayor Seth Low in 1901. Commissioner Woodbury has promised to solve the problem of final disposition, if possible.

ORGANIZATION.

The organization of the street cleaning department is as follows: At the head is a commissioner, who receives a salary of seven thousand dollars per year. Besides administering the affairs of the force it is the duty of the commissioner to advertise and let all contracts, which, if above one thousand dollars, to be valid, must be ratified by the board of estimate and apportionment. The commissioner appoints his own deputies and the uniformed force who work under them. The uniformed force consists of a superintendent, an assistant superintendent, a superintendent of final disposition, and an assistant superintendent of final disposition, and district superintendents. Under these last officers there are foremen, inspectors, hostlers, mechanics, sweepers, and drivers. The charter of New York provides for three thousand one hundred sweepers and sixteen hundred drivers at two dollars per day. The employes of the department from the commissioner down wear uniforms. Those whose duties require them to direct the uniformed force, dress in serviceable dark gray cloth. The sweepers wear white duck over-alls and jackets, which they own themselves and are required to keep clean. The white suits cost one dollar and a quarter each; the sweeper's whole outfit four dollars and sixty-three cents. The drivers dress is brown canvass. All wear helmets.

There are recognized lines of promotion, and rules and regulations for the guidance and discipline of the force. A court of appeal, consisting of the general superintendent, the assistant superintendent, and the superintendent of final disposition, sits each week to hear grievances and redress urjust punishment imposed by subordinate officers.

For convenience of operating the force, the city is divided into districts, and the districts into sections, the responsibility for which rests with district superintendent and section foremen, respectively. A section has about seven miles of streets.

Finally, there is associated with the street cleaning department what is known as the "juvenile league," an organization of children who assist the department by carrying on missionary work in the line of cleanliness and order in the streets and homes of the poor.

PROPERTY OF THE DEPARTMENT.

The property of the department consists of horses, stables, carts, dumps, scows, cremators, and various appurtenances. The horses are generally of fair stock, and the rules of the department require that they shall be well and humanely treated. Abuse of his horse by a cartman is punishable by discharge at the first offence. The animals are kept in fair condition and have roached manes and square trimmed, but not docked, tails. The carts are of two principal patterns, each being hauled by one horse. The type of cart used to haul street sweepings, ashes and garbage (the three principal divisions into which the refuse of the city is divided) consist of a tight metal body swung high upon two wheels. Paper and similar rubbish is collected in low-bodied carts which have a capacity of four cubic yards to the top of their sides. The stables of the department are situated in different parts of the city convenient to the work.

The docks where the refuse of Manhattan and Bronx and Brooklyn is dumped into scows for final disposition, consist generally of two storied, uncovered piers. The upper story has a broad platform for the carts which is reached by an inclined runway. Along the side of the wharf are usually ranged scows for garbage and others for dry refuse. Garbage is dumped from the second story of the dock into a proper barge. Italians then overhaul the garbage and extract bones and such other objects as are salable. Loads of refuse other than garbage are dumped upon the floor of the second story where the refuse is overhauled and the salable articles extracted. The first floor of the dock is used as a storage place where the pickers hoard and pack their findings for market.

The city owns two self-propelling, self-dumping scows. It has rented Barney dumpers at thirty to thirty-five dollars per day.

FINAL DISPOSITION WORKS.

The refuse of New York amounts roundly to three million cart loads per year. About ten per cent. of this is garbage. It has been estimated that the output of garbage is one hundred and forty-seven pounds per person per annum. The garbage of Manhattan and Brooklyn is taken to Barren Island where it is converted into salable products by the New York Sanitary Utilization Company under contract with the city. The contract for Manhattan dates from August, 1901, and is for two hundred and thirty-two thousand dollars per year; it will run for five years.[1] There was formerly a five years' contract for practically the same amount of garbage at one hundred and eighty-nine thousand nine hundred and ninety dollars per year. The difference in the cost is due to the fact that bids for the reduction of the city's garbage were not called for until too late to insure active competition.

Barren Island is twenty-five miles from the southern extremity of Manhattan, by sea route taken by the scows. The island is situated on the south shore of Long Island. It has been estimated that one hundred thousand people spend the summer within ten miles of these works. The contract capacity of the plant is a thousand tons of garbage per day. The reduction process is covered by the Arnold patents.

The garbage carried from the city in open scows, after arriving at Barren Island is unloaded and carried to the reduction works by mechanical conveyors. The reduction process consists in cooking garbage for eight hours under pressure of steam, after which it is pressed and its grease extracted. The digestors are steel cylinders measuring five and one-half by fourteen feet and are said to hold ten tons each. The residue after the grease and water are extracted is used as a basis for fertilizers.

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[1] A five year contract for the final disposition of the garbage of Brooklyn was let June 20, 1901, at $47.990.00 per year, the garbage being disposed of at the Barren Island plant. A contract for the collection of Brooklyn garbage was let also the price being $8.250.00 per month, the contract runs one year.

When using garbage from Manhattan, Brooklyn and the Bronx, the Barren Island plant is said to utilize one thousand five hundred tons per day. The works are in duplicate and represent a considerable investment. No contract has yet been let by New York for the disposition of garbage covering a longer period than five years.

Numerous complaints have been made by property owners in the vicinity of Barren Island that offensive odors emanate from the works which have an injurious effect upon the health of citizens and the value of property for a distance of ten miles from the plant. The chief annoyance is due to the familiar caramel odor universally associated with garbage reduction works. The odor escapes from the garbage after it has been cooked and could be overcome by an efficient system of ventilation, whereby the offensive matters could be carried with a current of air to a furnace and destroyed.

In 1899, five garbage furnaces of the Dixon type were put in operation in the outlying boroughs of Richmond and Queens, to dispose of the refuse of those localities. The capacity of each was designed to be twenty tons per day, and the builder contracted to dispose of the garbage at forty-five cents per ton. Coal was to be used as fuel to assist incineration. These plants were worked by contract with the city until they were purchased from Z. F. McGill, February 1, 1900. Since that time the city has operated the furnaces. The average cost of disposition of garbage in the McGill plants varies considerably. For the period of February, March and April succeeding their purchase by the city, the average cost of disposition of garbage was $1.23 per ton. In 1901, the plant at Port Richmond treated from twelve to fifteen tons of garbage per day, at an average cost of about one dollar per ton.

Ashes, etc., have been used to fill land at Newton Creek, Coney Island, Elizabethport, Craven's Point, Jersey Flats, Shooters' Island, Hastings, Steinway and Riker's Island. Formerly such material had been dumped to some extent in the harbor, but since September 1, 1900, the war department has required that no dumping be permitted inside of Sandy Hook Lightship.

COLLECTION OF REFUSE.

In Manhattan and the Bronx, refuse is collected by the men and carts of the department; in Brooklyn, the garbage is removed by contract between the city and a utilization company. In other parts of the city smaller contractors haul the refuse. Most of the refuse other than garbage is dumped into vacant lots. Household wastes collected by the street cleaning department are divided at their points of origin into several classes to facilitate removal and final disposition. Most householders provide metal receptacles for their "garbage" and in these places the remains of meats, vegetables, fruit, or as these matters are technically defined, "all materials any part of which has been used or intended for food." In another metal barrel, "dry ashes," which consist of ashes, broken glass, grass, tobacco stems, metals, etc., are placed. The third division of "refuse" is composed of house sweepings, papers, old boxes, rags, etc. This last assortment is known as refuse. Different carts are used by the department for the separate duties of collecting the three kinds of waste. All household refuse is placed outside the house within the stoop line. Paper and refuse are removed when a red placard bearing the letters P. R. in monogram is displayed in the basement window.

Garbage is removed at intervals of from twice a week to every day, according to the season and locality. Ashes are collected less frequently.

The streets are all swept by hand. One sweeper cares for about one-third of a mile of streets per day. Some streets are swept once a day and some four times a day. Street dirt, if in quantity, is swept into piles and removed by carts. If in small amounts it is gathered into coarse bags or cans carried upon a light frame work with wheels. This the sweeper pushes before him as he sweeps. The bags are carried away or emptied into carts which make rounds especially for this purpose. It is estimated that the comparative difficulty of cleaning streets paved with different kinds of standard pavements are: asphalt, 100; granite, 150; Belgian, 160; brick, 100; wood, 100, and cobble, 400.

The street cleaning department has contracts whereby part of the street sweepings are taken by a railroad company for the supply of farmers.

A refuse burning plant for the consumption of dry wastes has been begun at the foot of Forty-seventh street, North river. A contract for a refuse destructor in the Bronx has been let by the department of health. Details concerning these works are not yet available.

COST OF THE DEPARTMENT.

The expense of the street cleaning department according to the budget for 1902 will amout to nearly six million dollars. This will make the street cleaning department cost the city more than any other of the seventeen departments of government, except the police department and the department of education. It exceeds the combined cost of the department of water supply, the department of sewers, the department of buildings and the department of highways.

The amount of work to be accomplished may be understood from the fact that in Greater New York there are over seventeen hundred miles of paved streets and seven hundred and seventy miles of unpaved streets. The total area of Greater New York is 326.90 square miles. The population of five boroughs is 3,437,202. It may be said that in no other city is so much territory and so large a population served by a single authority on refuse disposal.

The chief items of expense, as shown by the annual reports of the commission of street cleaning, are:

1. Sweeping; by which is meant the collection of dirt, etc., from the pavements.
2. Carting; which includes the hauling of all refuse material, such as garbage, ashes, paper, and other like refuse.
3. Final disposition; which means the reduction of garbage by contract, dumping of ashes and other dry refuse to fill land.
4. The removal of snow and ice from the streets of heaviest traffic.
5. Administration.

The yearly expenses reported by the street cleaning commissioner by no means represent the total cost of keeping the streets of New York in a sanitary and orderly condition.

For a more satisfactory service than that given by the street cleaning department, many residents employ private carters to remove their garbage. The refuse of hotels, restaurants and apartment houses is generally removed by private contract at the expense of property holders. Nearly every residence block in Manhattan Borough has its private ashman. In 1897 it was estimated that there were thirty-eight wagons of forty-eight tons daily capacity engaged in the swill business in Manhattan. The number is now much larger.

The street cleaning department does not remove dead animals from the streets. This is done by contract with a scavanger who receives forty-one thousand dollars per year for his work from the department of health for Manhattan alone.

The street cleaning department does not sprinkle the streets. This is done by a private corporation known as the Street Sprinkling Association. The association is paid for watering the streets by residents and property owners.

The street cleaning department does not remove manure from stables. This is done by private contract under permits issued by the department of health.

These items all add appreciably to the actual cost of disposing of the city's refuse, and are, moreover, unequally distributed among the tax payers. The system which permits many interests to exist in collecting and disposing of wastes is also objectionable, for the reason that it fails to insure harmony and coöperation of action. Thus the street sprinkling association and the street sweepers are often at cross purposes to the injury and expense of both.

From practical and scientific investigation it has been found that the garbage of New York should be worth two dollars and forty-seven cents a ton; street sweepings five to thirteen cents a ton; paper refuse, four dollars and eighty cents a ton.

SNOW REMOVAL.

The removal of snow from the principal thoroughfares is accomplished by contract and is supervised and supplemented by the street cleaning department. Usually upon the fall of one inch or more of snow the contractor for snow removal is notified by the street cleaning department to begin work. Immediately a large force of shovelers and carts are employed, sometimes as many as six thousand carts being engaged in carting snow.

The snow collected from the streets is taken to the water fronts and dumped into North or East river. The cost of snow removal from Manhattan and Brooklyn for 1901 to 1902 was reported to be six hundred thousand dollars. This branch of the work of the street cleaning department is not and never has been even measurably satisfactory.

CONCLUSION.

The present outlook is not wholly encouraging for an early solution of the problem of final disposition of the wastes of New York. As the department of street cleaning is the result of a public agitation aroused by extravagant methods of administration, so further agitation and investigation are demanded if the department is to be fully and consistently developed.

The spirit of industry and self-respect among the laborers, which developed under Col. Waring, is not observable to-day. Until the street cleaners are made to regard their calling as respectable and honorable and are protected from political methods of management, the problem of disposition of the city's waste economically cannot be accomplished.

There are two great needs in the department of street cleaning: The first is that the recommendations of Mayor Grant's committee be lived up to; the second need is for an expert investigation of the problem of final disposition.

Among the lines of investigation which promise to lead to desirable ends are the following:

1. Is it advisable that one head should supervise and be responsible for the operations of street cleaning and final disposition in the large and diverse territory of Greater New York? or, should authority be invested in a number of officers with restricted areas, each officer to be held responsible for the conditions of street cleaning and refuse disposal in his territory?

2. Should the duty of removing offal and stable manure be added to the existing duties of the street cleaning department?

3. How can the garbage and other refuse of New York be finally disposed of to the greatest profit and least inconvenience of the citizens?

   (a) Should the city own its disposal plants?
   (b) Is cremation or reduction advisable?
   (c) Where should such plants be located?

4. Should the street cleaning department have charge of sprinkling the streets?

5. Are the present annual expenses sufficient for a proper conduct of the department?

6. Should the commissioner of street cleaning be a member of the board of health?

7. By what method of co-operation can the assistance of other departments, notably the police department, be secured, in order to facilitate the work of the street cleaning department?

8. Is the present system of employing laborers advantageous?

9. Can mechanical appliances be made use of to replace part of the hand labor now expended in cleaning the streets?

10. What improvements can be made for the disposal of snow?

STREET HYGIENE IN VERACRUZ AND CITY OF MEXICO.

By Dr. MANUEL S. IGLESIAS, Veracruz, Mexico.

In Veracruz the garbage from houses is placed in wooden boxes, generally cases which contained wine bottles, and also in empty petroleum cans. They are taken up every twenty-four hours in carts built for the purpose, which traverse the streets of the city until filled. They are then taken to special dumps located outside the city, and in a direction opposite to that of the prevailing winds. The garbage boxes remain outside the houses and on the edge of the sidewalks until collected by the carts which may arrive sooner or later. Some of these carts are covered; others are not. The streets and squares of the city are swept daily and the dust placed into suitable carts of the same nature as those used for the house garbage. This last service is somewhat imperfect, for the squad of street sweepers is insufficient for the needs of the city, and the lack of water in the city does not permit of the sprinkling of the streets.

Some years ago, when the city was surrounded by walls, it covered a surface relatively small and contained a population very large and compact. In those days the neatness of the streets, with their paved surfaces and sidewalks, was well known; and even at the present day the reputation for neatness still exists outside its limits among other cities in the Republic. This special care was due to the wealth of the municipality and to the limited number of streets and squares; for a long time squads of sweepers have had no other duties than to sweep and care for the streets.

The city in former times relied upon a spring, the water of which was then of sufficient volume to flow in the gutters by the sidewalks and to be utilized for sprinkling, carrying to the sea the waste material from houses and inhabitants, excepting night soil and garbage. The former was deposited into vaults (cess pools) which were periodically cleaned; the latter was removed as above described.

When the walls were demolished twenty years ago the crowded population began to spread over the lands adjacent to the city, largely increasing its area; and this area has grown progressively until at the present time it is four or five times greater than when the walls existed. The new part of the city is built up principally of frame houses, owing to the necessity of utilizing the area without overcrowding, and owing to the social and economic conditions of the people, which demanded rapidity of execution and small cost.

As was to be expected, the municipal income did not grow with the same rapidity; and though some improvements have been made in regard to garbage disposal, they have not been in proportion to the growth of the city. The ancient conditions which made the city one of the cleanest of the Republic have ceased to exist, and this is the apparent reason for the lack of neatness at the present day.

The greater proportion of the streets outside the ancient walled boundary are without pavements or sidewalks, and are sandy and dusty in dry weather, converted into mud and mire during the rainy season, and are at all times objectionable and neglected. The spring whose water was once used for sprinkling the streets has dried up. Since the work of improving the bay interferes with the delivery into the sea of the water from the gutters, these have now become stagnant with the wastes they contain notwithstanding that efforts are made to clean them every night. These conditions give the city an appearance of filthiness and neglect.

Notwithstanding this sad condition, things will soon change for the better, for we are now building a network of sewers and collectors which will deliver beyond the limits of the "Bay" all wastes of the inhabitants by water carriage. We are likewise engaged in securing a water supply for a population three times larger than the present one.

When these works shall have been finished we shall undertake the paving of streets and squares (plazas) in accordance with the most modern methods, for the sanitary authorities are diligently watching these matters and recommend to the executive authorities all the means leading to the improvements of the hygienic condition of the city.

The work done in the streets of Mexico, the capital of the Republic, is more in conformity with the precepts of modern sanitary science. Special wagons daily traverse the city streets, and are announced by the ringing of a bell. These wagons collect the house wastes progressively, and no garbage remains along the streets for indefinite periods, as happens in Veracruz. The wagons are covered and carry their contents to dumps which are located outside the city limits and in a direction contrary to that of the prevailing winds. All the city streets are sprinkled and swept twice a day. In special places this is done by squads of sweepers, paid by the municipality, who have no other occupation than to attend to the cleaning of the streets. These men use brooms and metallic scrapers, which are drawn along the pavement and collect any matter that may lie upon or adhere to the ground, in a manner similar to that done in New York and other

American cities. The street cleaning, in fact, leaves nothing to be desired.

In a portion of the city the inhabitants are obliged every morning and afternoon to sprinkle and sweep the dirt on the ground in front of their premises to the middle of the street. When the paving of the entire street is finished, municipal service will be extended throughout these districts.

The drainage works of Mexico will be finished this year, as well as a complete system of collectors and sewers. Very soon the entire city will be paved with asphalt, which is already used on many streets. This work will be actively prosecuted, for the municipality has contracted with construction companies for the execution of this work.

REPORT OF THE COMMITTEE ON ANIMAL DISEASES AND ANIMAL FOOD.

By D. E. SALMON, D. V. M., Chairman, Washington, D. C.

Your committee on animal diseases and animal food respectfully report as follows with reference to subjects which have received their attention during the time that has elapsed since the last meeting of the Association.

TUBERCULOSIS.

The last report which this committee had the honor to submit discussed the important question of the relation of bovine tuberculosis to the public health, reviewed the data available at that time, and concluded that the weight of evidence was overwhelmingly against the assertion of Koch that cattle are insusceptible to human tuberculosis, and that man is insusceptible to bovine tuberculosis. As anticipated in that report, numerous investigators at once instituted researches, and the result of the experiments are now being made public. Your committee feel that the question is of such general interest, and indeed that its elucidation is so essential to the proper performance of the duty of the sanitarian, that they venture to review it again, in the light of the most recent information attainable. There are some apparent inaccuracies in the paper which Koch read before the British Congress on Tuberculosis, to which your attention is invited. He says:

"Even in my first circumstantial publication on the etiology of tuberculosis I expressed myself regarding the identity of human tuberculosis and bovine tuberculosis with reserve."[1]

A reference to the paper which Koch mentions will show that he expressed himself as follows:

"It is not the peculiar structure of the tubercle, not its lack of blood-vessels, not the presence of giant cells that will give the solution, but rather the proof of the tuberculosis bacilli, whether it be in the tissue by means of staining reaction, or whether it be by means of culture upon coagulated blood serum. This criterion being adopted as a foundation principle, according to my investigations, miliary tuberculosis, caseous pneumonia, caseous bronchitis, intestinal and glandular tuberculosis, bovine tuberculosis, spontaneous and inoculation tuberculosis of animals must be declared as identical."

Again he says: "The tuberculosis of domesticated animals, especially bovine tuberculosis, undoubtedly forms another source of infection with tuberculosis. By this is also characterized the position which the care of health is to occupy in the future in consideration of the harmfulness of the meat and of the milk of animals suffering with bovine tuberculosis. Bovine tuberculosis is identical with human tuberculosis and therefore a disease transmissible to man. It is, therefore, to be treated just like other infectious diseases transmissible from animals to human beings. However great or small may be the danger which results from the consumption of meat or milk affected with bovine tuberculosis, it is present and must, therefore, be avoided. It is sufficiently well known that meat affected with anthrax is eaten by many persons and often for long periods of time without harm, and yet no one will draw the conclusion from this that the traffic in such meat is to be permitted."[2]

It is hardly necessary to add that the question appears to have been treated, in the paper mentioned, without any reservations whatever, and that the statements of the identity of human and bovine tuberculosis were emphatic and unequivocal. Of course, it is allowable for an author to change his views with the discovery of additional facts, but he may not cite his earlier paper in support of the latter contention when the language of the earlier paper gives, as in this case, an opposite impression.

Again, with reference to the results of the earlier investigators of the subject he states:

"If one studies the older literature of the subject, and collates the reports of the numerous experiments that were made in former times by Chauveau, Guenther and Harms, Bollinger, and others, who fed calves, swine and goats with tubercular material, one finds that the animals that were fed with the milk and pieces of the lungs of tubercular cattle always fell ill of tuberculosis, whereas those that received human material with their food did not."[1]

In your committee's last report[3] quotations were made from Chauveau's paper showing clearly that in his experiments cattle were successfully infected with human tuberculous material by ingestion, by intravenous injection, and by subcutaneous inoculation. From all of these experiments he concluded that the human tubercular virus acts on the bovine species exactly like the tubercular virus which comes from the bovine species itself.[4]

Bollinger,[5] in 1879, and Sidney Martin, in 1895,[6] also obtained successful results with bovine animals, one by inoculation into the peritoneal cavity, the other by feeding sputum. Your committee have not been able to consult the papers of Guenther and Harms, who were also cited by Koch, but whatever their results may have been, it is sufficiently demonstrated that the older investigators were not unanimous in finding that "the animals that were fed with the milk and pieces of the lungs of tubercular cattle always fell ill of tuberculosis, whereas those that received human material with their food did not." What is most striking in a review of these earlier investigations is the variability of the results. Chauveau found human as virulent as bovine material for bovine animals. Martin found human sputum much less virulent than bovine material, while others failed entirely in the effort to infect bovine animals with human tuberculous material.

It would seem that the cause of this lack of harmony in the results of experiments of the same character should have been suggested by the researches of Vagedes,[7] who in 1896, studying twenty-eight human cultures, discovered one of these which was much more virulent than the others, and which was more virulent than either of two bovine cultures used in the investigation.

Theobald Smith says in regard to this work "It is somewhat remarkable that this exceptional culture of Vagedes, described in a work done under the direction of Koch himself, was wholly ignored by the latter."[8]

Lartigau,[9] in 1901, published results from which he concluded that tubercle bacilli of widely different virulence may exist in different cases of human tuberculosis. One culture obtained by him was extremely virulent for guinea pigs and rabbits, and had morphological and cultural peculiarities which closely suggested the bacillus tuberculosis of bovine derivation as described by Smith.

Koch[1] says that in his experiments "In some cases the tubercle bacilli or the sputum were injected under the skin, in others into the peritoneal cavity, in still others into the jugular vein. Six animals were fed with tubercular sputum almost daily for seven or eight months; four repeatedly inhaled great quantities of bacilli, which were distributed in water and scattered with it in the form of spray. None of these cattle (there were nineteen of them) showed any symptoms of disease, and they gained considerably in weight."

For the discrimination between human and bovine bacilli he recommends the subcutaneous inoculation of bovine animals. Investigators who have since endeavored to elucidate the subject have therefore endeavored to infect animals with human material by any one or more of these methods which failed in the hands of Koch and his assistants. It is very interesting to study their results.

RECENT RESEARCHES WITH REFERENCE TO THE COMMUNICABILITY OF HUMAN TUBERCULOSIS TO ANIMALS.

Ravenel,[10] of the Pennsylvania Live Stock Sanitary Board and a member of this committee, in 1898, fed four calves with human sputum. Postmortem examination proved that all had become affected with tuberculosis, the lesions in two being quite extensive. In March, 1901, he obtained material from the mesenteric glands of a child which died of tubercular meningitis. The bacilli from this material have proved very virulent for bovine animals. One calf inoculated intravenously with a suspension of a culture of this bacillus showed marked illness during life and died in seventeen days. Both lungs were found thickly studded throughout with miliary nodules. The bronchial and mediastinal glands were much enlarged and soft, the former showing small areas of caseation. The liver was soft and friable and several nodules were visible on its surface.

A second calf was inoculated into the peritoneal cavity with a similar suspension of a culture of this bacillus. This animal died in twenty-seven days, after showing an abnormally high temperature from the eighth day. It was greatly emaciated, having lost 37 pounds in weight. The central and posterior portions of the lungs showed many areas deep in color and solid to the feel. These were thickly studded throughout with minute grayish nodules. The suprasternal lymph glands were as large as goose eggs, and contained cheesy areas. The mediastinal glands were much enlarged. The peritoneum was everywhere enormously thickened and practically converted into a tuberculous mass. The omentum and liver contained many nodules.

A cow was inoculated intravenously with only 2½ cc. of a suspension of a similar culture and died in seventeen days. The lungs were thickly studded throughout with miliary nodules.

A second culture obtained from the mesenteric glands of a child and which in its manner of growing and microscopical appearance led the investigator to consider it a typical human culture, was found to be unexpectedly virulent when tested upon dogs. A calf inoculated intravenously with suspension of a culture was killed on the 46th day, death then appearing imminent. The anterior lobes of both lungs were thickly studded with minute nodules averaging one millimeter in diameter. The bronchial and mediastinal glands were enlarged, and scrapings from the cut surfaces showed an enormous number of tubercle bacilli. Many yellow nodules were seen in the mediastinal glands. In the liver were many minute nodules, sections of which showed large numbers of tubercle bacilli. The spleen showed round-cell infiltration, giant cells, and many tubercle bacilli. There were also white areas in the kidneys with round-cell infiltration and many bacilli.

In the Bureau of Animal Industry, a number of virulent cultures of tubercle bacilli have been obtained from human sources. A few of these are worthy of notice in this connection. De Schweinitz,[11] of the Biochemic Division, isolated a tubercle bacillus from a mesenteric gland of a child affected with tubercular peritonitis. This bacillus when grown on egg medium corresponded to the bovine type. A calf received intravenously 5½ cc. of a suspension made from an egg culture. This animal died in twenty days of generalized tuberculosis. The lungs were a mass of small tubercles, while the bacilli were demonstrated in liver, spleen, kidneys, mesenteric and mediastinal glands. The same investigator obtained a culture from another child affected with peritoneal tuberculosis. This culture also corresponded to the bovine type when grown on egg medium. A calf inoculated intravenously with 5 cc. of a suspension of egg culture died in seventeen days. The autopsy showed generalized tuberculosis, numerous bacilli being demonstrated in the lung, liver, spleen, and mediastinal glands. A steer was inoculated subcutaneously with a piece of tuberculous tissue from this child. The animal was killed at the end of six months, and the autopsy showed the flank and shoulder glands enlarged, caseous and calcified. There were numerous excresences upon the border of the lung and upon the pleural surface of the diaphragm. Bacilli were demonstrated in these excrescences and also in the glands. Both of these cultures are therefore very virulent for bovine animals when given by intravenous inoculation, and one at least is virulent when injected subcutaneously.

A calf previously tested with tuberculin, as was done with all the larger animals used in these experiments, was inoculated subcutaneously in the side of the neck with 5 cc. of a suspension of a culture of this bacillus. This animal presented the ordinary signs of tubercular infection and being emaciated, weak, unable to rise and in a dying condition was killed on the 31st day. The autopsy showed generalized tuberculosis. The prescapular gland and mediastinal glands were enlarged and tubercular, the lungs were filled with tubercular nodules, and the liver was affected to an almost equal extent. The omentum showed many tubercular nodules, and the kidneys a small number.

Mohler,[11] of the Pathological Division, has obtained three cultures of bacilli from human sources which, because of their virulence for the species of animals on which they have been tested, are also worthy of mention. One of these that had its source in the mesenteric gland of a girl that died of tuberculosis, appears more virulent for goats and cats than the first culture of Ravenel's above mentioned. A goat inoculated subcutaneously was in a poor condition and growing thinner at the end of 137 days. A cat inoculated in the same manner died in 104 days of pulmonary tuberculosis. A rabbit was in poor condition at the end of 137 days. The bacilli cultivated on dog serum averaged 2 microns in length, some slightly curved and beaded. This bacillus was of the human, type but when recovered from the cat it was markedly reduced in length and slower in growth.

Another bacillus obtained from the mesenteric gland of a boy affected with tuberculosis is shorter, averaging from 1.2 to 1.5 microns, is beaded and grows rapidly and luxuriantly. A goat inoculated subcutaneously with this culture died in 37 days with miliary tuberculosis of the lungs involving the axiliary and prescapular glands. A dog inoculated in the same manner after 59 days was thin and becoming emaciated, and had an ulcer at the point of inoculation. A cat similarly inoculated was in poor condition at the end of 59 days. The third germ obtained by Mohler is particulary worthy of attention because it was isolated from human sputum. It is, when grown on dog serum, 1.7 to 2.2 microns in length, slender, beaded and curved. A goat inoculated subcutaneously died in 95 days of pulmonary tuberculosis. There were also tubercular lesions at the point of inoculation and in the prescapular gland. A cat inoculated subcutaneously died in 23 days of generalized tuberculosis. A rabbit inoculated in the same manner died in 59 days of pulmonary tuberculosis. A bovine culture was used for comparison with the cultures just described, and killed a goat in 55 days, as compared with 37 days for the second human culture and 95 days for the third human culture. The bovine germ also killed a rabbit in 69 days as compared with 59 days for the third human culture. This work also shows that it is not a difficult matter to obtain very virulent tubercle bacilli from human sources, and that some of these are just as virulent as bovine bacilli.

At the British Congress on Tuberculosis, Thomassen[12] reported an experiment in which a calf was inoculated in the anterior chamber of the eye with a pure culture of a tubercle bacillus isolated from a case of tuberculous arthritis in man. When killed after six weeks it was found that both lungs contained numerous miliary tubercles and some gray fibrous tubercles of larger size. The path of the infection from the eye to the lung could be traced by the condition of the subparotideal, cervical, mediastinal and bronchial lymph glands of the same side.

De Jong[13] reports as a result of a series of inoculations that seven bovine animals; namely, two calves, six months old; three steers, two years old; one 18 months, and one calf, seven or eight months old, all became tuberculous by the injection of tubercle bacilli of human origin; consequently all the bovine animals used in the experiment were infected by human bacilli. The disease produced was serious and very extended in only one animal, with four others it had a retrogressive tendency, and in the two remaining cases it was progressive. He concludes that these results demonstrate that bacilli isolated from the human body or from sputum are capable of causing tuberculosis of cattle.

Delépine[14] recently stated in a discussion on the relationship of human and bovine tuberculosis that he had been able to obtain from the human subject bacilli just as virulent as the bovine tubercle bacillus, but in most the human tubercle bacillus was less virulent.

Orth[15] made experiments with three calves, three swine and three goats. The infectious material was obtained from a case of phthisis cavernosus by inoculating a guinea pig and then obtaining cultures from this animal from brain and other media. In the experiments bouillon cultures of the organism were used and in some cases tuberculous organs from rabbits. One calf inoculated into the lung through the trachea gave an entirely negative result. The second, inoculated between the abdominal muscles, showed only a local caseous abscess. The third which was inoculated in the peritoneal cavity with two pieces of tuberculous kidney from a rabbit, died on the 26th day of a general tuberculous peritonitis. The serous membranes were covered with miliary and submiliary tubercles. The great omentum had nodules scattered throughout and small nodules were visible on the serous membrane of the intestine. The mesenteric, portal and retrosternal lymph glands were much enlarged and showed gray and white nodules upon section. There was no doubt but that a progressive fatal tuberculosis was produced in this calf by material originating in the human subject. Of the three pigs, two of which were inoculated intraperitoneally with the bouillon culture, gave negative results. The third, inoculated through the trachea into the lungs, was killed after five and one-half months, and a large number of miliary and submiliary tubercles were found in the inferior lobe of the right lung and there were many yellowish tuberculous nodules in the thymous gland and in the membranes around it. One of the goats inoculated through the trachea into the lung with the bouillon culture showed a number of subcutaneous nodules near the point of inoculation, a large caseous nodule in the thymous gland, and a number of yellowish gray nodules in the inferior lobe of the right and left lung. The second goat was inoculated intraperitoneally with a small piece of lung from a tuberculous rabbit. The animal was killed after five months and a number of nodules were found in the muscles and subcutaneous tissue at the point of inoculation. There were also a number of small grayish translucent nodules scattered through the omentum, on the diaphragm, and in the neighborhood of the lymph glands; a nodule about the size of a pea was found on the anterior border of the liver. The third goat was inoculated with a piece of tuberculous spleen the size of a pea placed in the peritoneal cavity. On autopsy a number of tubercles were found around the point of inoculation. There was a large group of caseous nodules on the adjoining surface of the stomach, and numerous caseous nodules in the omentum, many of these latter having small pedicles. Tubercle bacilli were easily demonstrated and there was no doubt as to the tuberculous character of the lesions nor that they were produced by human tubercle bacilli.

Stenström[16] inoculated eight calves in various ways with tuberculous sputum rich in tubercle bacilli. Of these the following showed positive results: No. 224 inoculated intraperitoneally with 5 cc. sputum. When killed after six months the autopsy showed many calcified nodules upon the omentum and an enlargement of the glands connected therewith. Many miliary tubercular foci were present in the liver and the posterior lobe of the right lung showed hypertrophy of the connective tissue on its serous cover. No. 820 inoculated in the lungs with 20 cc. when killed after four months showed many tubercular growths on the costal and diaphragmatic pleura, and tubercular nodules in two mediastinal glands. No. 883 inoculated intratracheally on October 8 with 6 cc. and on November 8 with 10 cc. of sputum, was killed March 17 and showed partially calcified nodules in the posterior and anterior mediastinal glands, and also in the bronchial glands. Stenström disagrees with Koch and states that he found it quite easy in his experiments to infect cattle with human tuberculosis.

Fibiger and Jensen [17] attempted to answer the question as to the intercommunicability of human and bovine tuberculosis by selecting from the various hospitals of Copenhagen those cases of tuberculosis in which the autopsy indicated a primary intestinal infection. They report in all five cases of tuberculosis in the following subjects: 1, a 42-year-old woman; 2, a 11-year-old girl; 3, a 6-year-old boy; 4, a 19-months-old girl; and 5, a 4-months-old boy. A 3-months-old calf was inoculated in the thoracic cavity with a suspension made from a mesenteric gland, of case No. 1. This calf remained apparently in good condition and was killed at the end of six months. Two miliary tubercles and a few fresh pearl nodules were found on the pleura. A 3-months-old calf was inoculated with a suspension made from the spleen of a guinea pig which had been inoculated with a mesenteric gland from case No. 2. When killed at the end of the fifth month there were found a large number of small red growths and about twenty small fresh pearl nodules on the omentum. On the diaphragm there were some small nodules and excrescences and one small excrescence on the pulmonary pleura. A calf was inoculated intraperitoneally with a suspension made from the spleen of a guinea pig which had been inoculated from case No. 3. When killed after about five months the omentum was found filled with tuberculous growths and showed countless small nodules of varying size. There were some small nodules found on other portions of the peritoneum and the surface of the spleen, liver and diaphragm was covered with small pearl nodules. A calf was inoculated subcutaneously on the right side of the neck with a suspension made from a mesenteric gland of case No. 4. When killed at the end of the third month there was a large caseous mass at the point of inoculation, the neighboring glands were large, caseous, and calcified, the lungs had scattered through them many small miliary nodules, and there were a number of excrescences attached to the pulmonary pleura. The bronchial and posterior mediastinal glands were enlarged, hard, and contained fresh, caseous, and calcified nodules. On the surface of the liver there were ten small pearl nodules. Miliary tubercles were scattered through the liver and spleen, and the omentum was filled with fine, diffused, red new growths. Two 10-days-old calves were inoculated subcutaneously on the side of the neck with a suspension made from a mesenteric gland from case No. 5. Both of these animals died within three weeks from secondary infection. Nevertheless there developed a considerable local tuberculosis rich in bacilli. From calf No. 2 a third calf was inoculated subcutaneously on the right side of the neck. A large abscess formed at the point of injection, and the calf being in a dying condition was killed at the end of about two months. The autopsy showed in addition to the local lesions tuberculosis of the adjacent lymph glands with many new growths on the costal and diaphragmatic pleura, and a few on the pulmonary pleura. The lungs contained a countless number of small nodules in some portions so close together that the air was practically absent. The bronchial and mediastinal glands were enlarged and caseous; the liver, spleen, and kidneys had miliary tubercles scattered through them. The intestinal wall was very rich in fresh nodules and small superficial ulcerations. The mesenteric glands were enlarged and tuberculous. The omentum was rich in small new growths. A cow about 10 years old was inoculated on the left side of the neck with material from calf No. 1, and developed an abscess at the point of inoculation. When killed at the end of four months, in addition to the local abscess, the neighboring glands were enlarged, caseous, and calcified. Miliary tubercles were scattered through the lungs, a nodule the size of a pea was found in the posterior mediastinal gland, and a small number of miliary tubercles in the liver. The authors say that it is thus seen that in the three cases of tuberculosis in children the bacilli present were virulent, some of them in the highest degree for calves, and it is probable that the disease in children was caused by bacilli which came from cattle. In the five cases there were found bacilli having the greatest variation in virulence for calves from those which were entirely avirulent to those which were slightly virulent, virulent, and of the highest virulence. They conclude by stating that Koch announced that one could distinguish between human and bovine tuberculosis by inoculating calves subcutaneously. If this is true, then they are of the opinion that three of the cases just cited must be considered as "Perlsucht," and the idea that tuberculosis of cattle is not virulent for men is disproved.

Max Wolff[18] reports inoculation from a case of primary tuberculosis of the intestines in man which he considers fully meets all the requirements demanded by Koch for the experimental solution of this uncommonly important question. Two guinea pigs were inoculated with diseased spleen. Seven or eight weeks afterwards they gave unmistakable evidence of being tuberculous and were in consequence killed. In each animal both lungs were studded with nodules of the size of a pin head, the liver contained numerous submiliary nodules and a swollen, cheesy gland was located at the hylus of this organ. The spleen was studded with nodules, many of them exceeding the size of a pin head. A calf was inoculated in the side of the neck with 12 cc. of sterilized water in which bits of a lung and spleen from the guinea pig had been macerated. Microscopic examination of this material showed tubercle bacilli to be present in very scanty numbers. Eighty-three days after inoculation the calf was killed. It had become considerably emaciated. The autopsy revealed very severe changes at the point of inoculation and characteristic pearl disease in the internal organs. Upon the surface of the pleura were numerous pedunculated tumors ranging in size from a flax seed to a cherry, and between these were hyaline miliary tubercles. The lungs showed many grayish tubercles, and the pericardium was covered with them. The mesentery contained countless small tumors, many of them pedunculated. The capsule of the spleen was studded, and the liver showed numerous miliary tubercles in its pulp and on its capsule. Each kidney bore five or six grayish tubercles the size of flax seed upon its outer surface. The virulence of the germs isolated from this calf was further proven by inoculations upon the guinea pig. The author thinks that it is here proven that pearl disease of cattle may appear in man.

Nocard[19] states that cattle are not refractory to human tuberculosis and that it is easy to give the proof of this. If they are inoculated in the arachnoid cavity with a few drops of a feeble culture of the human bacillus they succumb in less than a month and the autopsy reveals lesions of tuberculous meningitis absolutely identical to those of children. A calf five months old, inoculated in this way August 2, succumbed August 28. A portion of its piamater, infiltrated with tubercles, killed in six weeks the guinea pig which received it in the peritoneum.

Arloing[20] reports experiments with three different cultures of human origin. One of these had been in his laboratory since 1896. It was capable of infecting guinea pigs and rabbits by subcutaneous inoculation. He inoculated, by intravenous injection, with an emulsion of this culture, a heifer, a young calf, two sheep, and a kid. On autopsy he says that the calf and the sheep presented a superb eruption of tuberculous granulations in all parts of the lung, principally in the anterior lobe. With the kid the lesions were more diffuse and so extensive that the lungs scarcely collapsed when removed from the thorax. The heifer showed signs of pleural disease, for very marked excrescences were found on the borders of the lung and on their diaphragmatic surface. A small number of tubercles were found scattered through the lung. With the second culture he inoculated intravenously a calf, two sheep, and a goat. The autopsy demonstrated the existence of tuberculosis of the lungs in all of these animals. With the calf there were found a multitude of small young tubercles and the subpleural granulations slightly raised in the serous membrane were easily seen. With the sheep the lungs had but little tendency to collapse. There were many subpleural granulations and small tubercles throughout the parenchyma. With the goat the lesions recalled those found with the sheep, but were less confluent. With an emulsion of the third culture he inoculated a young bull, two sheep, and a goat by intravenous injection. The bull died at the end of 32 days. It was considerably emaciated, having lost 35 kilograms. The anterior and median lobes of the two lungs were studded with tubercles of various sizes, the largest being the size of a millet seed. The bronchial and esophageal glands were enormous, weighing 400 grams. In the lungs of the sheep there were perceived by the touch an enormous number of small subpleural tubercles. On section there were also seen many tuberculous granulations in the depth of the parenchyma. With the goat the lesions were similar to those found in the sheep, except the granulations were smaller.

Behring[21] reports an experiment in which human sputum was injected into a guinea pig, and a culture from this animal's spleen was used for inoculating a goat. An emulsion of the spleen of the goat was passed through a series of guinea pigs and a culture obtained from the spleen of the third guinea pig was inoculated into a calf by intravenous injection. The calf died after four weeks directly from the tuberculosis thus produced. At the autopsy the lungs were found in the red stage of hepatization. From these organs tubercle bacilli were recovered in large numbers. A fairly numerous collection of subpleural nodules were present which were yellowish in color and had reached in some instances the size of peas. Tubercle bacilli were recovered readily from these nodules. The bronchial glands were swollen and tubercle bacilli were proven present within them. The liver contained fatty infiltrations, and the spleen was darkened but scarcely enlarged. Tubercle bacilli were recovered from it, but were not numerous. The pronounced malignity of the virus in this instance is worthy of particular notice because it concerns bacilli derived from human sputum, although they had been passed through the goat. A cow was also inoculated with a tuberculosis culture of human origin after a single passage through a guinea pig. The inoculation was made intraocularly on Nov. 26, 1901. On Jan. 14, 1902, the affected eye was enucleated and a pure culture was obtained from it after one passage through a guinea pig. The cow did not die at once as a result of the inoculation, but has been gradually losing ground and was, at the time the report was made, in a condition of chronic disease so serious that the final outcome could not be foretold.

Dean and Todd,[22] who have recently carried out certain experiments to ascertain whether the tubercle bacillus of human origin undergoes any marked change in virulence for the bovine species by passage through certain other animals, say that "the experiments show that the human tubercle bacillus is by no means innocuous to the calf, as the control animal injected directly with sputum contracted an extensive glandular tuberculosis," also "as mentioned above, with reference to the infection of the pig, an important result was obtained, as the experiments conclusively prove that this animal is capable of contracting a rapidly fatal general tuberculosis as the result of inoculation with the tubercle bacillus of human origin."

These investigations are now sufficiently numerous and sufficiently harmonious in their results to establish the following conclusions:

1. The bacillus tuberculosis found in human tuberculosis differs greatly in its pathogenic powers as obtained from different cases.
2. Two types of tubercle bacilli may be obtained from man, namely, one which is somewhat difficult to cultivate, which grows slowly, and the bacilli of which are short, stubby and free from beading—the so-called bovine type; and a second, which is quite easily cultivated, grows rapily, is longer, thinner and inclined to show beaded and curved forms—the so-called human type.
3. Virulent cultures may be obtained from both of these types which when inoculated, according to the methods used by Koch and upon the species of animals specified by him, produce progressive and fatal tuberculosis.

4. The contention that human tuberculosis can not be transmitted to asses, sheep, goats, swine, and especially to cattle have been completely disproved.

5. Koch's failure to produce tuberculosis in the animals named with bacilli from human sources was probably due to the use of bacilli of low pathogenic power.

THE TRANSMISSION OF BOVINE TUBERCULOSIS TO MAN.

Some interesting observations have also been made during the year with reference to the transmission of bovine tuberculosis to man. Krause reports the case of a laborer who was brought to the clinic in order to ascertain whether the trouble in his right arm was due to infection, and if so, of how long standing. The man stated that he was a butcher, whose duty it was to remove the diseased parts of cattle killed for food. Three years previously he ran a splinter of wood from a table into his right thumb and immediately afterwards removed the hide from a sick cow. Soon his arm swelled and pained him; later it was covered with discharging ulcers of various sizes. Incisions were made in the arm, but without causing complete healing; afterwards larger incisions were made which were successful. Pieces of gland and skin from the arm proved, on examination, to be tuberculous. According to the man's statement he is of a healthy family, 30 years old, and had not previously suffered from disease. The lungs were sound, as were all the other internal organs, and his mind was clear. The reporter thinks there is no doubt whatever that this was a case of inoculation tuberculosis traceable directly to the splinter-wound on the thumb.

Ravenel[23] reported, in his address at the British Congress on Tuberculosis, the case of his assistant who was inoculated with bovine tuberculosis, and from whose tissues bacilli were recovered after 58 days, which retained the activity of the original bovine virus.

Quite recently Spronck and Hoefnagel[24] record the following case: In May, 1900, a veterinarian accidentally wounded, with a knife, the finger of a butcher who was assisting him in inspecting the tuberculous organs of a cow. The wound healed promptly, but was followed in a few days by tumefaction and the formation of cracks in the skin. The skin of the finger became thickened and blue in color and the cracks covered with little scabs. In February, Professor Nareth extirpated the affected portion of the skin together with the tumefied cubital ganglia. The tuberculous nature of the cutaneous lesions, as well as that of the hypertrophied ganglia, was easily recognized both by microscopic examination and by the inoculation of guinea pigs. A calf previously tested with tuberculin was placed in a newly constructed stable in which no animal had been kept and inoculated with an emulsion made from the spleen of one of these guinea pigs. Five days after the inoculation a swelling was noticed, and in a few days the superficial cervical ganglia were visibly tumefied. The animal soon exhibited constitutional symptoms, remained lying down most of the time, and drank excessively. The swelling increased rapidly and the animal was slaughtered 57 days after the inoculation. At the autopsy there was found at the point of inoculation on the right side of the neck a tuberculous granuloma the size of a fist, enclosing at its center a large cavity filled with a soft cheesy mass. The neighboring superficial ganglia were found granulomatous, hypertrophied, and hard. The pleura and the visceral folds presented numerous tubercles varying in diameter from 1 to 6 mm. The tissue of the lungs was also the seat of numerous tubercles of similar size, each showing an inflammatory areola. The bronchial and mediastinal glands were hypertrophied, and on section showed caseous, non-calcified contents. Similar conditions were found in the organs generally throughout the body. Microscopic examination demonstrated that the tuberculosis foci contained abundant giant cells and numerous tubercle bacilli. Finally, two guinea pigs which had been inoculated with material from the hypertrophied cervical ganglia, one in the peritoneum and the other beneath the skin, died of tuberculosis. The bacilli had consequently retained their virulence for a period of twenty months, during which they lived in human tissues. These observations, according to the authors, leave no doubt as to the possibility of the infection of man with bovine tuberculosis, and they hold that cases of contagion from the use of milk, butter, and meat of tuberculous cattle are not so rare as Koch and Baumgarten maintain.

Lassar[25] was impelled by Koch's statements to closely examine the records of his patients during the past decade to determine how many of those that were suffering from verrucose tuberculosis of the skin of the hands could have been infected by means of injuries received in handling tuberculous meats. He found 34 cases of tuberculosis following wounds in 108,000 patients, but only four of this number were butchers. He later questioned and examined 365 men who were employed in abattoirs and found 7 suffering from inoculated tuberculosis, while 3 others must be considered as possibly affected. This shows two to three cases to each 100 men employed in the abattoirs, and only one-third of one per thousand among persons otherwise employed.

Hüls[26] reports a case of a miller's family in good circumstances near Manderschied, composed of the miller, his wife, five sons, and two daughters, all herculean in stature and boastful of strength and health. Consumption had never occurred in the families of either of the parents. The mother became affected with pulmonary catarrh, which aroused a suspicion of tuberculosis, but after a few months she was again healthy. During the following year the 13-year-old daughter became ill of pulmonary tuberculosis and died. The same year an 18-year-old son died, and the following year a 23-year-old son. Two years after her first illness the mother sickened and died, then followed the death of a 16-year-old daughter, then the father, and finally the third son, all of pulmonary tuberculosis. For one of the two remaining sons a tuberculous abscess of the finger was treated, which healed. These two men are still living. Hüls thinks that the view of contagion from the sick members of the family can not be held, since there was practically no contact. These cases appeared subsequently to the introduction by the miller of a herd of Simmenthal cattle on his farm. These cattle were all practically infected with tuberculosis and it was scarcely possible to dispose of the meat on account of its infected condition. Many of the carcasses were returned on account of being tuberculous, so that later he was compelled to sell the cattle without a guarantee as to their condition.

Considering these facts in connection with those of similar bearing previously reported, it may be said that the transmission of bovine tuberculosis to man is fairly well established. The apparent inoculation of a human subject with bovine tubercular material, the development of a tubercular process at the point of inoculation, the isolation from the human tissues months afterward of a tubercle bacillus having the characteristics of the bovine bacillus, constitute a chain of evidence that is conclusive.

INVESTIGATIONS BEARING UPON INTESTINAL INFECTION.

Heller[27] has made an investigation as to the comparative frequency of primary intestinal tuberculosis. He says that it is well known that tuberculosis frequently appears after diphtheria, and he therefore investigated to determine first how often tuberculosis was present before the attack of diphtheria, and second how often the primary lesions were located in the intestine.

Sections were made from 714 victims of diphtheria and among these 140, or 19.6 per cent., were found to have an associated affection of tuberculosis in various organs.

2— 1.43 per cent. showed primary intestinal tuberculosis.
8— 5.7 per cent. showed primary intestinal and mesenteric gland tuberculosis.
33—23.5 per cent. showed primary mesenteric gland tuberculosis.
———
43—30.7 per cent. of all the tuberculosis cases.

43— 6.0 per cent. of all the diphtheria cases.
10— 7.1 per cent. had mesenteric gland tuberculosis with affection of other abdominal organs.
———
53—37.8 per cent. of all tuberculosis cases.

53— 7.4 per cent. of all diphtheria cases.

In addition to these there were six other cases affected with tuberculosis of the intestines or of the mesenteric glands, together with a tubercular affection of the lungs.

With 30.7 per cent. of so many tuberculosis cases showing primary lesions in the intestines or mesenteric glands, the argument against infection by ingestion of tuberculous food which was raised by Koch and based upon German statistics, appears to be greatly weakened.

Gottstein,[28] in a recent statistical study relative to the etiology of tuberculosis, concludes that among children in Berlin the mortality of those at the breast is but slightly more than half as much from tuberculosis as among those otherwise nourished, the proportion as he finds it being as 6 to 10. He also finds, in studying the statistics of 23 large cities and university cities, where the returns are especially reliable, that whereas there has been a reduction of the mortality at all ages from tuberculosis, this reduction has been distinctly less in children under 15 years of age than in persons over that age. From this he concludes that "the thought is not to be set aside that in the etiology of tuberculosis in children and in grown people, very important differences must occur. There must be assumed for the infection of children another source than for that of grown people, and it certainly is natural to conclude from the difference established statistically that the source of children's tuberculosis is to be sought partly in nourishment with milk containing tubercle bacilli, while the decrease since 1894 is to be traced back to the better general and individual prophylaxis in the care of milk."

An examination of the vital statistics of Massachusetts and Michigan, the only states from which sufficiently complete records were received, indicates a tendency of the same nature in the development of tuberculosis in the United States. In "The Vital Statistics of Massachusetts, 1856-95," the two years 1856-57 are contrasted with the two years 1894-95. In the two years first mentioned, for the period under five years of age, there were 46.3 deaths from pulmonary phthisis to 1,000 deaths from all causes. In the two years last mentioned there were but 15.6 deaths from pulmonary phthisis to 1000 deaths from all causes, or one-third as many. This is a very gratifying reduction in the phthisis death rate. Let us see now how it is with other forms of tuberculosis. In the two years first mentioned there were in the same age class 70.7 deaths from other forms of tuberculosis to each 1000 deaths from all causes; and in the two years last mentioned there were 96.2 deaths from other forms of tuberculosis to each 1000 deaths from all causes. That is, in this 40 years there appears to have been an increase of 36 per cent. in the forms of tuberculosis other than phthisis in the class under 5 years of age, while there was a reduction in the mortality of phthisis at all ages of about 45 per cent. The vital statistics of Michigan show that in that state during the years 1870 to 1884, inclusive, taking the class under 5 years of age, there were for each 100 deaths from consumption 81.8 deaths from other forms of tuberculosis; during the years 1885 to 1897, inclusive, there were 104.3 deaths from other forms of tuberculosis to each 100 from consumption; while during the three years 1898 to 1900, inclusive, there were 263.3 deaths from other forms of tuberculosis to each 100 from consumption. Why this tremendous increase in mortality from other forms of tuberculosis as compared with consumption at what has well been called the milk-drinking age of life?

Another important question which was discussed by your committee in its last report has reference to the possibility and frequency of tubercular infection through contaminated food when the primary lesions are elsewhere than in the abdominal cavity. The frequency with which infection may occur through the walls of the upper air passages, and particularly those of the naso-pharynx and pharynx, was shown. From recent experiments it appears that there is very likely another channel by which such infection may readily occur. It is a common observation that tubercle bacilli may penetrate the intestine and produce tubercles in the mesenteric glands without causing any lesions at the point where they pass through the intestinal wall. It has been frequently held, however, that they could not enter the thoracic duct and the blood vessels without first passing through and being arrested by a lymphatic gland.

Dobroklonski[29] in 1890 conducted some investigations from which he concluded that tuberculosis may certainly affect the organism by the digestive tract. For this infection to occur it is not necessary that there should exist a lesion of the intestinal wall, an epithelial desquamation, nor any local modification whatever, nor an interior inflammatory process. The tubercular virus (bacilli as well as spores) may easily traverse the completely normal epithelial lining of the intestine. The tubercle bacilli as well as their spores, unless they remain a long time in contact with the intestinal wall, are not capable of causing inflammatory processes in this wall, nor modifications of the epithelial coat.

In 1895, Desoubry and Porcher[30] made investigations with dogs to learn if microbes of various kinds may pass through the intestinal wall and enter the thoracic duct. They took chyle directly from the cistern of Pecquet, and also blood from the portal vein. In both cases they demonstrated the presence of bacteria. The species were not determined, but they observed cocci, bacteria, and bacilli. They say in conclusion, it seems to follow from their experiments that microbes may follow the lumen of the chyle vessels. Every time that the animal received either fat or milk they constantly found bacterial species in the chyle. They ask, may we not infer from this that the microbes pass into the chyle vessels by means of the globules of fat?

Quite recently Nicolas and Descos[31] have experimented in a similar manner after feeding tubercle bacilli to dogs. These investigators fed a soup composed of from 1 liter to 1 liter and a half of milk, according to the size of the animal, from 80 to 100 grams of fat, and about 100 grams of bread to dogs which had fasted 24 to 36 hours. The animals were killed from three to three and a half hours later. The chyle for examination was taken from the cistern of Pecquet or the thoracic duct. After each experiment a complete autopsy was made to assure the authors that the animal did not possess any tuberculous lesion, and that the intestine was perfectly normal. The presence of tubercle bacilli was demonstrated by the coloration of the microscopic preparations and by the inoculation of guinea pigs. Positive results were obtained by both methods. The authors conclude as follows: The essential fact brought out by these experiments is that three hours after the ingestion of tubercle bacilli, the chyle and the lymph of the thoracic duct may contain bacilli and even virulent bacilli in a sufficient number to cause tuberculosis in the guinea pig. They think it quite interesting to know that this may occur under conditions which approach quite closely those of a child fed with tuberculous milk.

To recapitulate, recent investigations show that

(1) While the bacilli of human tuberculosis are often of feeble virulence for cattle, there is a considerable proportion of cases in which these bacilli are virulent for cattle as well as other animals.

(2) Bacilli of both the bovine type and the human type have been obtained from cases of human tuberculosis.

(3) Bovine bacilli introduced into the human tissues by accidental inoculation have lived, multiplied, and produced disease at the point of inoculation, and have been recovered after a considerable time with their vitality and virulence unimpaired.

(4) Various statistical studies indicate that a considerable proportion of the cases of human tuberculosis, and particularly with children, originate through the ingestion of the bacilli with contaminated food.

It appears to your committee, therefore, that the conclusions of Koch, announced at the British Congress on Tuberculosis, to the effect that bovine and human tuberculosis are different; that human tuberculosis can not be conveyed to cattle, and that man is insusceptible to bovine tuberculosis are disproved and should no longer have weight with sanitarians. The evidence brought forward indicates that greater care should be exercised to prevent human infection with bovine tuberculosis, and particularly to guard children from tuberculous milk.

RABIES.

Rabies is an extremely dangerous disease, which continues to exist in many parts of the country without any very vigorous or systematic efforts being made to repress it. In the past, doubts have been raised by some pathologists as to the specific nature of rabies and particularly as to the transmission of this disease from dog to man. Inoculation experiments have, however, settled this question beyond any reasonble controversy, and it is time for progressive sanitarians to accept this fact and to turn their attention to measures of prevention. The one method of controlling this disease which the experience of the world has shown to be most efficacious, namely, the muzzling of dogs in the infected districts, can not usually be enforced in this country because of the ignorance of our people concerning the disease, and the popular prejudice against any measure which appears to cause the least discomfort to these animals. In considering this matter, it appears that the medical profession has contributed somewhat to the popular errors concerning this disease, by uncertainty within its own ranks and by failing to take as decided a stand as it should upon a question of such great importance. This being the case, it is all the more necessary that the profession should now accept unreservedly the facts brought out by scientific investigation, and teach the public that there is such a disease as rabies; that it is a common disease; that it is communicable from dog to man, and that the muzzling of dogs is the only efficient method of eradicating it.

Koch's reference to this disease in his address before the British Congress on Tuberculosis has apparently been lost sight of because of the many other questions which he discussed that were more closely connected with the object for which the congress was called. His remarks with reference to hydrophobia, however, tersely expressed the experience of Germany and are worthy to be remembered by every health officer and by every physician who desires to hold and to teach correct views. Koch says:

"Hydrophobia, too, is not void of instruction for us. Against this disease the so-called protective inoculation proper has proved eminently effective as a means of preventing the outbreak of the disease in persons already infected, but, of course, such a measure can do nothing to prevent infection itself. The only real way of combating this pestilence is by compulsory muzzling. In this matter also we have had the most satisfactory experience in Germany, but have at the same time seen that the total extermination of the pestilence can be achieved only by international measures, because hydrophobia, which can be very easily and rapidly suppressed, is always introduced again year after year from the neighboring countries."

The state board of health of Minnesota had knowledge of eighty-five cases of rabies in that state in 1901, namely, ten dogs, seventy cattle, four hogs, and one horse. In 1902 to November 1, there were recorded in that state seventy-three cases of rabies in animals, namely, nineteen dogs, sixteen cattle, thirty-seven hogs, and one horse. During the latter period, four people were known to have been bitten by rabid animals, and one death resulted. This, however, by no means represents the total number of cases of rabies in the state during the period referred to, but only those which happened to come to the attention of the board.

The Bureau of Animal Industry, during the fiscal year ending June 30, 1902, made a positive diagnosis in twenty-five cases of rabies occurring in the District of Columbia or in the immediate vicinity. One of these was an unfortunate woman who was bitten by a dog, but who did not know that the dog was rabid and consequently did not take the Pasteur treatment. Nineteen cases were diagnosed in dogs, three in cows, one in a hog, and one in a horse. There are no measures enforced at the national capital for the suppression of this disease.

GLANDERS AND ANTHRAX.

There are some other diseases of animals communicable to man, which unfortunately are far too prevalent. One of these is glanders. This disease is widely distributed and measures for its control are either insufficient or entirely lacking according to locality. A considerable number of human lives are lost every year by infection with the glanders bacillus. Bracken reports that three cases have been reported from the laboratory of the Minnesota state board of health during the past six months. Case 1 was infected directly from a glandered horse. Case 2, a brother of the first-mentioned case, was infected in caring for his brother. Both of these patients died. Case 3 was infected in making an autopsy of a guinea pig inoculated from case 2. Infection apparently occurred through a slight cut on the finger. The first symptoms were suggestive of typhoid fever, but later three superficial and localized lesions developed, as did also a fourth point of localization still later. These were treated surgically and the patient is apparently progressing favorably. This series of cases illustrates the danger, not only to those who care for glandered horses, but also to those who care for human patients, and even to the laboratory workers who make the inoculation tests. It appears certain that there must be numerous cases of glanders in man which are never properly diagnosed, and which consequently do not appear as such in the vital statistics.

Outbreaks of anthrax appear to be becoming more frequent, and new centers of the disease are from time to time reported. The middle, western, and northwestern states, while not as severely infected as the lower Mississippi valley, often report the plague sometimes in animals, sometimes in men. The long period that this infection may remain active in certain soils, makes an outbreak in any locality a very serious matter, since it is never known when or how often the disease may reappear.

There should be great care in handling the carcasses of animals affected with either glanders or anthrax. All contaminated soil should be scraped up and buried with the carcasses, the whole being well covered with quicklime before the earth is filled in over them. Autopsies should be made with the greatest care to guard against infection, and the owners of affected animals should have the dangerous nature of these diseases fully explained to them. Health officers can do much to educate the public in these matters, and should seize upon the opportunity presented by an outbreak of disease to disseminate information while public interest is aroused.

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SANITARY MEASURES PROPOSED TO THE MEXICAN RAILWAY COMPANIES.

By Dr. E. LICEAGA, Mexico, Mex.

Before the supreme board of health of Mexico had received the report made by the chairman of the committee on "car sanitation," the writer in the name of said committee, addressed the different railroad companies of the Republic of Mexico, and now comes to the meeting to make known to you what was written then, and the replies received—which, up to the present, are as follows:

There have died in the City of Mexico last year, 2013 persons from tuberculosis. You know this affection is transmissible from a person affected with it to a healthy one. The transmission is done through the medium of the sputum, which is expectorated on the ground, furniture, mattresses, walls, etc., of any habitation. The sputum dries and becomes dust, and when swept or brushed or in any other way the air is set into motion, it carries the dust, loaded with tuberculosis germs. This dust is breathed by persons in the house and if there be any among them who are predisposed to it, they are liable to contract the disease. The most efficacious way to avoid this is to forbid that the sputum be allowed to dry and the best way to do this is to expectorate in a cuspidor that contains a disinfecting solution. The cars of the railroads are more unfavorable than the dwelling houses, for in the former, the germ of tuberculosis being mixed with the dust, and this being set in motion within an enclosed space, the transmission of tuberculosis from an infected individual to a healthy one, who is, however, predisposed to it, is facilitated.

The Pullman cars being sleeping apartments of very small capacity, the air soon becomes loaded with germs and is, therefore, unhealthy. The linens of these cars are changed daily. However, I do not know whether they go through a disinfecting process before being washed. The linens and blankets (the latter I do not believe are changed frequently) are exposed to contamination by tuberculosis sputum, this dries up and soon becomes dust, which goes to form a part of the atmosphere breathed by those occupying the beds, and exposes them to infection. Soon there will be a law forbidding any one to expectorate anywhere else but in a cuspidor, but for the present it is better to instruct the public and urge them to do this until the said law has been passed.

This series of considerations has decided me to address you in the name of the supreme board of health, and we beg of you that the measures hereafter stated may be kindly adopted:

First—Increase the number of cuspidors and furnish them with disinfecting solutions capable of destroying the tubercular germs.

Second—Place in conspicuous places placards written in large print in English and Spanish the following:

NOTICE.

"Please spit exclusively into the spittoons, as otherwise the germs of infectious diseases, especially those of tuberculosis, will be carried into the air."

Third—Until the paddings of the cars be changed for other more convenient drapery, a wise plan would be to disinfect the car every time it is unoccupied with a solution of formaldehyde that will not alter the conditions of the cloth nor its color.

Fourth—It would be well to disinfect all sheets and pillow cases before sending them to the laundry.

Fifth—Also be wise to disinfect the tapestries and couches.

Sixth—Keep the car as clean as possible, and especially the closets and urinals.

We hope that you will attend to the wish of the board, for the benefit of the passengers as well as for the welfare of the company you represent.

Fearing to be tiresome, I will not enclose all the answers received by the different companies, but in different terms, all said that our recommendations were very good, and that they would be put in practice, and that we must communicate any suggestion we thought to be useful. The companies from which we have received answers are as follows: Mexican Railroad, Mexican Railroad of the Isthmus of Tehauntepec, Chalco and Rio Frio Railroad, Hidalgo and Northeastern Railroad, Mexican International Railroad and Mexican National Railroad.

I am sure that all these companies will equally accept the circulars of the committee "on car sanitation," written by Professor S. H. Woodbridge.

REPORT OF THE COMMITTEE ON THE CAUSE, PREVENTION, PERIOD OF INCUBATION AND DURATION OF INFECTIOUS DISEASES: A GENERAL REVIEW.

By A. WALTER SUITER, A. M., M. D., Chairman,
Herkimer, New York.

It will be the purpose of this resumé to consider material facts concerning prevalent infections with special reference to their causation and prevention. In thus endeavoring to fulfill to some extent the duties imposed by the organization of this committee, free use will be made of individual statements and public reports, which have from time to time appeared from various sources.

The enormous growth of that branch of scientific inquiry which relates to the public health in recent years precludes the possibility of detail, and the writer hopes that the pardon of the members of the Association may be forthcoming if it shall seem that he has erred in his judgment as to the importance of the topics selected for presentation and comment.

In the last general review submitted by this committee somewhat extended reference was made to the epoch-making studies and experiments conducted by representatives of the United States Army in Cuba relating to the cause and prevention of yellow fever, the practical application of which gave promise of extraordinary results. Without doubt detailed statements with reference to the methods employed to apply the now immortal principles elucidated by the studies mentioned will be made through appointed channels during this meeting of the Association, but it seems proper that attention should be called herein to the great accomplishments which have been achieved, and which have attracted the admiration and favorable notice of the devotees of public sanitation throughout the entire civilized world. It may indeed be said that an unprecedented impulse has been given to sanitary science during the American occupation of the Island of Cuba. In a period of time almost incredibly brief that pest-ridden island, which was for centuries a constant menace to adjacent territory, has been transformed by the efficient administration of sanitary laws, together with the application of newly discovered innovations in science, to a condition of health that can be favorably compared to that of any country on the globe. Witness the last monthly report of vital statistics of the City of Havana by Major W. C. Gorgas, chief sanitary officer:

For the year 1898 when the American occupation began the death rate was 91.53 per 1,000; for 1899 it was reduced to 33.67; for 1900, to 24.40; for 1901, to 22.11; while the first four months of 1902 showed the hitherto unparalleled low rate of 20.68.

It should be observed that these results were obtained without the aid of a sewerage system and under many difficulties that do not ordinarily hamper the efforts at sanitation in more favored cities. The common measures relating to cleanliness and disinfection, however, were employed, and the efficient quarantine precautions as carried on by the Marine Hospital Service were at no time neglected. It is proper to state that the formerly prevalent ideas relating to the destruction of fomites were disregarded in the successful effort to bring about the extermination of yellow fever and malaria, in pursuance of the mosquito-borne theory which had been so brilliantly established.

The following excerpts from the eloquent summary of results by Dr. Gorgas when relinquishing to the officials of the new Republic his temporary charge may well be recorded here:

"The army took charge of the Health Department of Havana when deaths were occurring at the rate of 21,252 per year. It gives it up with deaths occurring at the rate of 5,720 per year. It took charge with smallpox epidemic for years. It gives it up with not a single case having occurred in the city for over eighteen months. It took charge with yellow fever endemic for two centuries. It found Havana feared as a thing unclean by all her neighbors of the United States, and quarantined against as too dangerous to touch or even to come near anything that she had touched, to the untold financial loss of both Havana and the United States. It leaves after careful study of yellow fever by its officers undeterred by personal risk. It has established the fact that yellow fever is only transmitted by a certain species of mosquito, a discovery that in its power for saving human life is only excelled by Jenner's great discovery, and as time goes on, it will stand in the same class as that great boon to mankind."

Although the acceptance of the belief that the mosquito *can* convey the infective element of yellow fever may be regarded as almost if not quite universal, and the evidence appears to be preponderatingly upon the side of those who insist that the contaminated *Stegomyia fasciata* acting as the host is the *only* agent that ever conveys the infection from man to man, there still remain some competent observers who declare quite as strongly that that question must yet be regarded as *sub judice,* in so far at least as the continuance of the ordinary methods of quarantine and disinfection is concerned.

Surgeon H. R. Carter (Bulletin No. 10 Yellow Fever Institute, Bureau of Public Health and Marine Hospital Service) sums up an exceedingly scientific disquisition upon this important matter thus:

"For the belief that yellow fever is conveyed in nature *only* by a host and doubtless a mosquito host, we have—

1. The analogy of other diseases conveyed by insect hosts.

2. That all facts observed about the propagation of yellow fever agree with the necessary deductions of this theory.

3. No other theory explains all the facts observed of its propagation." Dr. Carter adds to this the following: "The second postulate, may be, I think should be tested further by the collection of other facts to be compared with the theory; possibly on the lines given above, for obtaining direct negative evidence about this theory of conveyance. Yet the evidence already extant seems to the writer convincing and he is convinced that yellow fever is conveyed in nature *only* by a mosquito host of a particular kind or kinds."

On the other hand, Dr. Edmond Souchon, President of the Louisiana State Board of Health, in a recent article (Medical Record, 25th October, 1902) takes the ground that the epidemic infection of yellow fever naturally exhausts itself in from two to four years in a locality, and cites the history of such epidemics in New Orleans from 1817 to 1899 inclusive, together with other pertinent evidence, to prove the truth of his assertion. While admitting his belief in the transmissibility of the infection by mosquitos, he holds to the opinion that the extermination of mosquitos "*hastened the exhausting of the infection*" and believes "that Havana would have become free of yellow fever anyhow, and that the *actual exemption* is truly due to the quarantine work that prevented the introduction of new infection."

From the facts adduced in his article he declares it to be his opinion "that there is some other means of transmission" than by the mosquito.

We have then the postulates of these two distinguished students and observers, which are here introduced because they well illustrate the lack of universal acceptance of the exclusive insect-borne theory, and seem to be fairly representative of opposing opinions which stand for such diametric differences in the matter of practical sanitation. In the one case the quarantine restrictions usually practised would seem to be of slight importance, while the acceptance of the other proposition would make the destruction of the mosquito but one of the many agencies upon which to rely to control the disease and eradicate it from infected ports. Obviously this is a question of great consequence in an economic point of view.

As this report does not contemplate more than to simply state leading facts and events of current interest, and suggestively to promote the study of the same, no attempt will be made to discuss comparatively the arguments involved in this very weighty problem.

It may be remarked in passing that a late report by Dr. Carlos Finlay, Chief of the Sanitary Department under the new regime, indicates that no case of yellow fever has originated in Havana for the year ending September 30, 1902. It is said that such a statement could not have been made for a period of one hundred and forty years.

By natural sequence we have now to mention the fact that we are rapidly approaching a situation of affairs which bids fair to test the life-saving principles elucidated by the recent developments in the study of tropical infections. It is practically certain that the construction of the interoceanic canal across the Panama isthmus, which will bring both hemispheres into comparatively close commercial relationship, is about to be inaugurated. The history of the deplorable loss of human life by disease that was incident to a previous attempt by the people of another nation to accomplish this vast undertaking, and the similar experience during the construction of the trans-isthmian railway, leads to great apprehension of a possible repetition of those disastrous occurrences, and already the thoughtful interest of sanitarians has been aroused.

A very able and timely article upon this subject from the pen of Gen. George M. Sternberg, Surgeon General U. S. A. (Retired), a distinguished member of this committee, has recently been published (North American Review). This article is replete with most excellent advice regarding the great desirability of the establishment of a systematic sanitary service by the government with power to enforce the application of the improved methods of sanitation now available, and it would seem that the object of this reference will have been attained if the attention of the members of the Association is hereby directed to the discussion by Dr. Sternberg of the principles involved.

Taking for example the transcendent results of study and administration in Cuba, we may confidently hope that a renewed demonstration of the wonderful progress of sanitary science awaits us upon the Isthmus of Panama.

The prevalence of bubonic plague in various parts of the world continues to be a subject of great concern. The latest reports[1] indicate that this devastating pest prevails to a greater or less extent, and in varying rates of mortality, in the following countries: Africa, Australia, Brazil, China, Egypt, France (imported cases on steamships), Hawaiian Islands, India, Japan, Madagascar, Philippine

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[1] Public Health Reports, November 28, 1902.

Islands, Russia, South Africa, Spain (imported case on steamship), Turkey, and, we regret to state, in the United States. As has been the case for several years the disease is by far more widely distributed in India than in any of the countries mentioned, or all combined. The mortality rate continues to be appalling—in many instances approaching one hundred per cent.

The much discussed relationship of infected rats is still universally recognized as the principal (probably the only) means of conveyance in the matter of the causation of the disease. Official investigation by the examination of rats during the prevalence of the plague in Manila proved that five per cent. of all rats were affected by the disease. Vigorous means were employed to disinfect all vessels that were infected by rats and a general rat-killing crusade resulted in the abatement of the epidemic. At the present time only isolated cases are observed.

The study of the epidemiology, and especially the bacteriological diagnosis of this pestilence, is constantly being supplemented by the unremitting attention of skilled observers in various countries. Statements in a much quoted article by Dr. Balfour Stewart, Plague Research Laboratory, Bombay, may well be repeated here:

"The bipolar staining (of the bacillus) in stained specimens when the ends are found to be deeper stained than the center is by no means a constant phenomenon and cannot be relied on for diagnosis. Mr. Watkins-Pitchford considers it due to a greater collection or condensation of the protoplasm in these regions. He notes that the spots are readily visible in the living and unstained specimens, so they cannot be due to staining."

Again: "The bacillus is polymorphic; it is sometimes seen as a very short bacillus, almost like a coccus or diplococcus, at other times as a short stumpy bacillus with rounded ends."

The plague is a disease, primarily at least, of seaport towns and is generally conveyed by ship-borne rats. In many ports of the western part of the world the infection has thus appeared without gaining permanency or the development of epidemic characteristics. This has been largely due to the prompt isolation and disinfecting regulations employed. It will be noted that the latest reports show that a permanent foothold has been attained in but two occidental countries, viz.: Brazil in South America and the United States at San Francisco.

With reference to the persistence of this dreadful infection in San Francisco, a very peculiar and deplorable state of affairs is presented. The disease appeared in the so-called Chinatown of that city nearly two years ago. The genuineness of the cases observed was disputed. A conflict of authorities took place and much acrimonious discussion ensued, which resulted in a series of careful investigations by expert bacteriologists, who were sent there for the purpose. The confirmation by these experts of the diagnosis of the officers of the United States Marine Hospital Service has lead to a feeling of gravest apprehension among sanitarians who, familiar with the epidemiological history of such outbreaks, recognize the direful consequences of an ostrich-like policy in their management on the part of those whose official duty requires them to be ever mindful of the protection and preservation of the public health.

According to the latest issues of the Public Health Reports (October 31, 1902), the disease has been constantly present in San Francisco and vicinity since December 12, 1901, and save in a single instance every case reported has resulted fatally—a mortality rate of nearly one hundred per cent. For a short time past an increased headway has been observed, thirteen cases having been reported from September 9, 1902, to October 7, 1902, with an apparent tendency to spread to other towns in the vicinity.

When the fact is considered that it is the usual history of great epidemics of plague that the disease exists in an epidemically mild form for several years preceding the general outbreak, the full seriousness of this situation becomes manifest. It is said that in Calcutta, where from April 26, to September 13, of the present year, there were 1,487 cases with 1,479 deaths, the general outbreak was preceded by a form similar to that reported in San Francisco for two years. At a meeting of the representatives of the county and city boards of health of Indiana, together with representatives of the state boards of health of Ohio, Kentucky and Indiana, on April 25, 1902, a series of resolutions was unanimously passed beginning: "We view with surprise and alarm the acts of the Governor of California and the Mayor of San Francisco, in attempting to suppress the facts relating to the presence of bubonic plague in that city," and, after reciting various circumstances pertinent to that declaration and pointing out the duties of those who have authority in such premises, the resolutions conclude as follows: "Lastly, we call upon sanitary associations, boards of health, medical and scientific associations throughout the country to join with us."

At the last meeting of the Conference of State and Provincial Boards of Health of North America, held in New Haven, Conn., during the last week of October, 1902, part of a resolution adopted was the following:

"That the Conference of State and Provincial Boards of Health views with abhorrence the irretrievable disgrace of the present State

Board of Health of California, and pronounces the plague situation in California a matter of grave national concern."

It is believed that the foregoing statements will be sufficient to indicate the significance of this very commendable awakening. Whether this Association shall deem it consistent to formulate a similar expression of views will be for it to determine, but it would seem that this committee would have neglected a very material part of its duty had it failed to give space to a reference to this interesting state of affairs. Let it be kept in mind that the present status of modern sanitation forbids that mediaeval experiences shall be repeated in this enlightened land. There is a prevalent belief that the yellow races are peculiarly susceptible to this pest and that the whites are comparatively immune. Recent experience in Capetown,[1] where the white and colored races were equally afflicted—the Chinese and Malays least of all—militates strongly against the truth of that proposition. Let no false sense of security or mistaken views regarding commercial interests prevent the adoption and application of all means at hand to avert a threatened general visitation of this disease —the deadliest known to man.

Smallpox prevails to a greater or less extent in various parts of the world. It is a very significant fact that wherever the practice of vaccination is optional there is the greatest prevalence. The continued widespread epidemic of this disease in the United States is still a subject of much concern and solicitude. In accordance with the custom of recent years the admirable statistics of the Public Health and Marine Hospital Service are offered for comparison:

Total number of cases reported from December 28, 1901, to June 27, 1902	38,361
Total number of deaths	1,277
Total number of cases for the same period in 1901	30,710
Total number of deaths	552

It will be observed that a material increase in the number of cases was attended by a considerable increase in the rate of mortality—the percentage being a little more than 3.32 in the six months ending June 27, 1902, and 1.79 in the corresponding period in the preceding year.

It seems reasonable to believe that much of the doubt in relation to diagnosis, which was disputed to so great an extent in the early history of the epidemic, has by educational and observational experience been removed. It is possible that the increased number of cases reported may in some degree be attributable to that circumstance.

It cannot be believed, however, that there is any evidence of abatement in the prevalence of the disease in view of the fact, as shown by the above figures, that the increase in the number of cases reported amounted to more than twenty-seven per cent. It should be noted that by the determined efforts of the health authorities in many of the large cities such as Chicago, New York, Philadelphia, and others, in the adoption of vigorous means to secure the certain protection of vaccination, threatened general epidemics have been averted and practical immunity established.

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[1] Journal of Tropical Medicine, June 15, 1901. Editorial Journal A. M. A., July 13, 1901.

It is useless in this presence to offer any arguments to prove the efficacy of vaccination as the only means whereby the practical control of the current visitation of this disease may be accomplished. This fact has been so abundantly demonstrated by the experience of countries and municipalities that are favored by compulsory vaccination laws and their rigorous enforcement as not to require repetition here. The universality of this experience during the past century makes it seem surprising indeed that at this date a campaign of education is needed to secure the full benefit of an immortal principle, the application of which is a certain safeguard to the public health and commerce. It is said that even the benighted and superstitious Chinese are beginning to recognize the prophylactic value of vaccination.

The United States and Great Britain are notoriously behind Germany in this respect owing to popular indifference and the effective opposition of a zealous minority of the populations, who are able to contribute much towards the blocking of the wheels of scientific progress. A considerable impetus was afforded by the anti-vaccination propaganda during the past year by the occurrence of a few cases of tetanus following vaccination, which were easily explained by atmospheric and telluric circumstances and well known facts regarding the viability of the tetanus germ.

The absolute purity of vaccine virus, however, is a subject that should never be neglected until the highest ideals have been attained. A very interesting discussion of this important topic led to the following declaration by the Conference of State and Provincial Boards of Health of North America, at its recent meeting: "That in order to obtain the most satisfactory results, vaccine must be produced either by federal, state or provincial officials, or by private producers under the closest supervision of qualified government officials."

This committee would urge the importance of a well-directed and systematically organized effort to educate public opinion upon the general subject of vaccination. Much of the indifference and opposition is due to ignorance of its value, and, unquestionably, a large part of the fear and prejudice regarding it arises out of a knowledge of the unfortunate results of impure virus and carelessness on the part of both vaccinator and subject. Quite recently a very commendable movement in this direction has been inaugurated by the Imperial Vaccination League of England, having for its object the insertion of a provision in the proposed new Vaccination Act to require revaccination of the school children of that country, and the dissemination among the masses of information by means of literature, public meetings, etc. It has been suggested in regard to the latter proposition that a similar propaganda might be undertaken in this country and that "some philanthropist could do a great work with a very little money" in this field.[1] Here is an opportunity for practical benevolence. Who will make the sacrifice?

Without doubt the largest factors in the unusual spread of the current smallpox epidemic are the non-recognition of ambulant cases, and contact with infected articles—fomites. This emphasizes the signal importance of careful inspection of all eruptive skin manifestations by authority whenever observed, early diagnosis, prompt isolation and thorough destruction or disinfection of all inanimate articles, the possible contact with which might constitute a mode of conveyance. Some of the conclusions reached by Dr. John L. F. Sikes, Medical Officer of Health in London, in a valuable article[2] are "that smallpox is carried by the dirty linen of unrecognized cases and occasionally of recognized cases; that smallpox is also carried by persons brought into contact with infected cases, probably by the clothing, and that these contacts may also incubate, develop, and spread the disease and so reiterate the cycle."

In this connection reference may profitably be made to a remarkable contribution to the general subject of fomites (the importance of which is now so much disputed) by the studies of Drs. Viola and Morello, reported in the Bulletin of the Sicilian Society of Hygiene.[3] Some of the conclusions derived from their experiments upon clothing and other garments were—that each square centimeter of fabrics may contain from 915 to 571,962 micro-organisms pathogenic and otherwise; that the number corresponds with the length of time the garments are in contact with the body; that wool is a greater germ-bearer than cotton or silk; that the life tenure of pathogenic bacteria is shorter in clothing actually worn than when contaminated and hung in a wardrobe; and that they gradually attenuate, lose their virulence and die under all conditions. The final conclusion was "that garments are a potent means for the diffusion of infective agents, and that without special treatment pathogenic germs can retain their virulence in human raiment for a considerable but not indefinite period."

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[1] Editorial American Medicine, September 20, 1902.

[2] English Public Health Journal, January, 1892, quoted by Medical Record.

[3] Public Health Reports U. S. P. H. and M. H. S., August 15, 1902.

Previous to the American occupation of the Philippine Islands the occurrence of epidemics of Asiatic cholera in the far East, where it is to a greater or less extent endemic, was so remotely dangerous to our interests as to need but little consideration.

Owing to the presence of American troops and civilians in large numbers in those islands, and the consequent direct communication with the ports of the Pacific seaboard of the United States, the serious outbreak of that disease in the latter part of March of the present year in Manila became at once a subject of much concern.

The epidemic originated by the landing of infected vegetables from Canton, China, and rapidly assumed proportions of great gravity among the native population, spreading to so great an extent that from March 20, 1902, to September 19, 1902, the reported cases amounted to a total of 58,147 with a mortality of 40,707—truly an appalling record. This report by Dr. J. C. Perry, Chief Quarantine Officer, was accompanied by the following statement: "I consider a conservative estimate of the cases of cholera that have actually occurred in the islands since March 20, 1902, to be 75,000, with a mortality of 75 per cent."[1]

A considerable number of American soldiers became victims of the infection, but by the rigorous application of sanitary precautions a serious foothold among the troops has been avoided.

A quarantine detention of five days of all ships leaving for the United States was wisely ordered and as a consequence the disease has thus far been prevented from reaching our shores. Efforts of the most energetic character have been made by the medical officers of the army to prevent its spread among the provinces but without satisfactory result, on account of the ignorant opposition of the people to sanitary measures designed for their relief. Latest reports indicate that the epidemic is practically under control in Manila, but that it prevails with the same severity in the provinces.

There are still many undecided questions relating to the pathology, bacteriology and epidemiology of cholera, which the exceptional opportunities afforded the expert medical officers who have this epidemic under observation will doubtless elucidate.

The radical declaration of Prof. Koch at the London Congress on Tuberculosis regarding the non-transmissibility of animal tuberculosis to man, as was anticipated, evoked an extensive discussion.

Reference to a report of the last International Congress of Tuberculosis, held in Berlin, October 23-26, 1902, would seem to justify the belief that the great problem is no nearer solution than when first promulgated. This congress was attended by representatives of various countries including Prof. Koch, who took occasion to reiterate his assertions of one year ago. He was vigorously opposed by Prof. Nocard of France, and others.

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[1] Public Health Reports, October 24, 1902.

Since this question (of such great importance both from a nosological and economical point of view) of the intercommunicability of bovine and human tuberculosis was brought so strikingly to the attention of the hygienists of the world, a prodigious amount of experiment and study have been applied to effect its satisfactory settlement. It is to be regretted that such a result cannot be here recorded. The now oft repeated statement may properly be believed, however, that the prolonged discussion of this and associated subjects relating to the cause, prevention and treatment of tubercular consumption has been productive of a renewed and continuous interest in the public mind which cannot but be of lasting benefit, not only to the unfortunate sufferers themselves but to public sanitation as well. At no previous time has there been so great an outpouring of material aid in the promotion of means for the alleviation and cure of the victims of this dread disease. An almost untold number of sanatoria for indigent tuberculous cases have been established, both by public and private charity, and the helpful influence that such institutions exert in the matter of public education in hygiene is already in evidence.

PERIOD DURING WHICH CONTAGIOUS DISEASES ARE TRANSMISSIBLE AND SICK PERSONS ARE DANGEROUS TO HEALTHY PERSONS NEAR THEM.

By Dr. LUIS E. RUIZ, Mexico, Mex.

In order that the *prophylaxis* may be efficacious, it must act on the *place* and on the people, mediately or immediately.

Mediate results are accomplished through sanitation, individual healthfulness and vaccination; and the immediate results are: declaration, isolation and disinfection. The following simple considerations will only refer to isolation on account of a certain number of transmissible diseases, in consonance with the proposed question.

Isolation (when the disease requires it) in order to be efficacious must satisfy two fundamental conditions: first, it must be complete; and second, it must begin when the disease becomes transmissible and finish when the transmissibility ceases.

The importance of isolation, is conceded and also its practical difficulties and the necessity of not neglecting even the slightest detail.

If sufficient resources are at hand to obtain the first condition of isolation, it is necessary to look carefully to the second because it is the principal subject of the proposed theme.

Science and practice, observation and experience, have put beyond all doubt what refers to the period of incubation and period of contagiousness of the group of diseases to which I am going to refer.

Instead of presenting the study analytically, I am going to formulate synthetically the list that sums up the results of our knowledge.

"Disease."	"Incubation."	"Period of Contagiousness."
Whooping cough	4 to 14 days	8 weeks
Typhoid fever	8 to 14 "	8 "
Smallpox	12 "	6 "
Diphtheria	1 to 8 "	6 "
Scarlet fever	1 to 6 "	6 "
Measles	8 to 20 "	4 "
Wens on the throat	14 to 22 "	3 "
Varioloid	10 to 14 "	3 "
Cholera	1 to 5 "	2 to 3 weeks

The five years observation (1896 to 1901) in the elementary schools of the capital of the Mexican Republic (Mexico City) has confirmed, in regard to contagiousness, the foregoing conclusions with the exception of cholera, typhoid fever and diphtheria, for which I have not sufficient data.

I think I have answered the theme, and I only have you observe that if formerly, through ignorance, fear dictated the rules of defence against transmissible diseases, to-day, owing to the progress of human knowledge, science orders what we have to do in such cases.

A NOTE CONCERNING THE TRANSMISSION OF PATHOGENIC FUNGUS BY FLIES AND MOSQUITOES.

By Dr. JOSE P. GAYON, Mexico, Mex.

The notable work by Curtice, who was the first to suppose that the propagation of Texas fever was not rare by the boöphilis bovis (tick), a fact which was shown beyond doubt by the experiments of Smith and Kilborne; that of Grassi about the filaria recóndita; of Nott, of this beautiful city of New Orleans, who in 1848 showed in his work upon malaria many reasons in support of the modern theory of the mosquito; the masterly exposition of King, and the works of Manson, Grassi, Bignami, Pfeiffer and Koch, demonstrating that the mosquito serves as a vehicle for swamp fevers; and last those of Finlay, who since 1886 defended the theory of the mosquito in yellow fever, are all proven to-day by the celebrated experiments of Reed, Carroll, Lazear and Agramonte, and it is now an evident fact, that the transmission of these sicknesses have been caused by means of these insects.

Among the pathogenic fungi, Aubert in 1879 said that the louse was predisposed to propagate by contact of scabs because the germs adhere to the feet and hair of these insects, facts proven by the experiments of Dewevery, but, are these the only insects that carry from the sick to the healthy, pathogenic fungi?

This is the subject which for some time I have proposed to solve.

With this object in view I have directed my investigations to the common fly and the mosquito, because they are the insects most frequently in contact with our bodies, and which exist by thousands in our habitations.

The method which I have followed consists: First, in a search for these insects in divers places, in hospitals, habitations and streets, by means of a test tube thoroughly sterilized, the mouth of which is placed directly over them and immediately closing the mouth with sterilized cotton. This is easy for flies during the night, and for the mosquitoes in the day. It is then necessary to guard the body of the insect, and isolate the microscopic organisms found upon it.

In order to develop the fungi, always under the same conditions, and that the colonies may not vary, I have prepared as a means of cultivating them, the following formula:

Water	100
Pepton Chapoteaux	2
Glucose	3
Phosphate of lime	1
Gelatin	8

This should be slightly acid and reduced to a liquid state in a sand bath, then applied to the surface of the body of the insect and placed in an Erlenmyer flask, of a quart and placed in a stove heated to 37° centigrade. I captured sixty flies in the following places, 25 in my house, 20 in different hospitals, 15 in divers places, yards, streets, etc. By the same means I captured 15 mosquitoes *culex pungens,* of which ten were from different hospitals and five from my house.

Of the first sixty investigations all gave positive results developing different classes of fungi such as the *claviseps purpurea, el peronospera infestans, aspergilus glaucus, aspergilus niger,* etc., but in no case have I been able to find the *achorin de Schoenelein, el microsporum de Audouini* or any tricophytones. The crop of mosquitoes has been void in every case.

If one wished to draw any conclusions from these facts, it would be easy to fall into error for the reason of not having found pathogenic species for men, is not to say that in determinate circumstances these insects cannot be those which carry to the healthy the seeds of scabs; and before all it is supposable that this mode of contagion can be realized for the fact that a fungus is or is not susceptible of produced sickness, it is not an especial circumstance which prevents their adherence to the feet and wings of animals.

By all means I believe it would be a great public benefit, hygenically, that they should isolate the scabby in quarters free from flies and mosquitoes in the same manner as yellow fever patients.

Perhaps they will say that this class of contagion being rare this precaution, which demands especial services, is unnecessary, which entails extra expenses but if it is taken into account the duty of the hygienist is to work for the extinction of all classes of diseases one will agree that in treatment of the health nothing is useless.

REPORT OF COMMITTEE ON PUBLIC HEALTH LEGISLATION.

By U. O. B. WINGATE, M. D., Chairman, Milwaukee, Wis.

As is well known, for a number of years a strenuous effort has been made to provide for a National Department of Public Health, with a medical secretary in the cabinet of the President at its head, but no member of Congress could be found who would favor such a measure, the argument being that the cabinet was already too large and that no more departments would be favored, especially a department of public health. Efforts were made to create a bureau or commission, separate and apart from any other service, in the Treasury Department. Such a measure found expression in what was known as the "Spooner Bill," introduced and strongly advocated in the senate by Senator Spooner, and which has been several times approved by this Association. This measure, however, was sharply and persistently opposed by the Marine Hospital Service, and at the last session of Congress that service had a bill introduced to change the name of the Marine Hospital Service to that of a Public Health Service, and contained certain other provisions which it was hoped would satisfy those who favored the "Spooner Bill." The chairman of your committee visited Washington several times during the year, and the situation was carefully studied, and it was learned that in all probability the "Spooner Bill" could be passed by the senate, but that the friends of the Marine Hospital Service measure would not permit its passage by the house. On the other hand, the Marine Hospital Service measure could undoubtedly be passed by the house, but the friends of the "Spooner Bill" would not allow its passage in the senate, hence in order to secure any legislation a compromise was required. Representatives from various state boards of health and state quarantine officials met in Washington with the supervising surgeon general of the Marine Hospital Service, and the matter was carefully considered, and finally a compromise was effected. The friends of the "Spooner Bill" very reluctantly yielded to this pressure, but there was nothing else to do. Many of the friends of the "Spooner Bill" had become discouraged in the vain attempts to overcome the objections to the passage of the measure, and one by one began to yield to any compromise that would settle the difficulty and end the struggle. There was nothing to do but to yield to the pressure which was brought to bear upon them and your committee, and the bill was passed and became a law, changing the name of the Marine Hospital Service to that of the Public Health and Marine Hospital Service. A copy of that law is submitted with this report.

It will be observed that this law provides for an annual conference of the health authorities of all the states and territories and the District of Columbia, and that it shall be the duty of the surgeon general of the service to call a conference upon application of not less than five states or territorial boards of health, quarantine authorities, or state health officers. It will be seen by this provision that one of the most vital principles of the "Spooner Bill" has been attained, for it has been constantly maintained that in order to have a national health service operate harmoniously, representatives from the different states and territorial boards of health should have a voice in the adoption and execution of the rules and regulations affecting their respective localities. A broad and liberal policy, free from arbitrary rule, and coöperative with state health authorities, on the part of the surgeon general, will undoubtedly prove advantageous and strengthening to the work of the various state and territorial boards of health. It is hoped that this measure provides a foundation upon which may be built up a national health service, such as the intelligence of the people and the age in which we are living demand.

PUBLIC HEALTH AND MARINE HOSPITAL SERVICE OF THE UNITED STATES.

AN ACT TO INCREASE THE EFFICIENCY AND CHANGE THE NAME OF THE UNITED STATES MARINE HOSPITAL SERVICE.

Be it enacted by the Senate and House of Representatives of the United States of America in Congress assembled, That the United States Marine Hospital Service shall hereafter be known and designated as the Public Health and Marine Hospital Service of the United States, and the Supervising Surgeon General and the officers now or hereafter commissioned under the Act of January fourth, eighteen hundred and eighty-nine, entitled, "An Act to regulate appointments in the Marine Hospital Service of the United States," and acts amendatory thereof, shall hereafter be known as the surgeon general, surgeons, passed assistant surgeons and assistant surgeons of the Public Health and Marine Hospital Service of the United States. Nothing in this Act contained shall be held or construed to discharge any of the officers above named, or any of the acting assistant surgeons, pharmacists, and other employes of the Marine Hospital Service, or to deprive any officer of his commission or the benefits derived by longevity of service. The care of sick and disabled seamen and all other duties now required by law to be performed by the Marine Hospital Service, and all funds and appropriations now provided by law for use by the Marine Hospital Service and all properties and rights pertaining to said service shall be available for use for like purposes and in like manner, under the Treasury Department, by the Public Health and Marine Hospital Service.

SECTION 2. That the salary of the Surgeon-General of the Public Health and Marine Hospital Service shall be five thousand dollars per annum, and the salaries and allowances of the commissioned medical officers of said service shall be the same as now provided by regulations of the Marine Hospital Service.

SECTION 3. That commissioned medical officers, when detailed by the Surgeon-General for duty in the Public Health and Marine Hospital Bureau at Washington, District of Columbia, in charge of the administrative divisions thereof, namely, marine hospitals and relief, domestic quarantine, foreign and insular quarantine, personnel and accounts, sanitary reports and statistics, and scientific research, shall, while thus serving, be assistant surgeons-general of the Public Health and Marine Hospital Service, but their pay and allowances shall be the same as now provided by regulations of the Marine Hospital Service for officers in charge of said divisions; and the senior officer thus serving shall be the assistant within the meaning of section one hundred and seventy-eight, Revised Statutes of the United States; provided, however, that no such officer shall be detailed in charge of said divisions who is below the rank of past assistant surgeon.

SECTION 4. That the President is authorized, in his discretion, to utilize the Public Health and Marine Hospital Service in times of threatened or actual war to such extent and in such manner as shall in his judgment promote the public interest without, however, in any wise impairing the efficiency of the service for the purposes for which the same was created and is maintained.

SECTION 5. That there shall be an advisory board for the hygienic laboratory provided by the Act of Congress approved March third, nineteen hundred and one, for consultation with the Surgeon-General of the Public Health and Marine Hospital Service relative to the investigations to be inaugurated, and the methods of conducting the same, in said laboratory. Said board shall consist of three competent experts, to be detailed from the Army, the Navy, and the Bureau of Animal Industry by the Surgeon-General of the Army, the Surgeon-General of the Navy and the Secretary of Agriculture, respectively, which experts, with the director of the said laboratory, shall be ex-officio members of the board, and serve without additional compensation. Five other members of said board shall be appointed by the Surgeon-General of the Public Health and Marine Hospital Service, with the approval of the Secretary of the Treasury, who shall be skilled in laboratory work in its relation to public health, and not in the regular employment of the government. The said five members shall each receive compensation of ten dollars per diem while serving in conference, as aforesaid, together with allowance for actual and necessary traveling expenses and hotel expenses while in conference. Said conference is not to exceed ten days in any one fiscal year. The term of service of the five members of said board, not in the regular employment of the government, first appointed shall be so arranged that one of said members shall retire each year, the subsequent appointments to be for a period of five years. Appointments to fill vacancies occurring in a manner other than as above provided shall be made for the unexpired term of the member whose place has become vacant.

SECTION 6. That there shall be appointed by the Surgeon-General, with the approval of the Secretary of the Treasury, whenever, in the opinion of the Surgeon-General, commissioned medical officers of the Public Health and Marine Hospital Service are not available for this duty by detail, competent persons to take charge of the divisions, respectively, of chemistry, zoölogy, and pharmacology of the hygenic laboratory, who shall each receive such pay as shall be fixed by the Surgeon-General, with the approval of the Secretary of the Treasury. The director of said laboratory shall be an officer detailed from the corps of commissioned medical officers of the Public Health and Marine Hospital Service, as now provided by regulations for said detail from the Marine Hospital Service, and while thus serving shall have the pay and emoluments of a surgeon; provided, that all commissioned officers of the Public Health and Marine Hospital Service not below the grade of past assistant surgeon shall be eligible to assignment to duty in charge of the said divisions of the hygienic laboratory, and while serving in such capacity shall be entitled to the pay and emoluments of their rank.

SECTION 7. That when, in the opinion of the Surgeon-General of the Public Health and Marine Hospital Service of the United States, the interests of the public health would be promoted by a conference of said service with state or territorial boards of health, quarantine authorities, or state health officers, the District of Columbia included, he may invite as many of said health and quarantine authorities as he deems necessary or proper to send delegates, not more than one from each state or territory and District of Columbia, to said conference; provided, that an annual conference of the health authorities of all the states and territories and the District of Columbia shall be called, each of said states, territories and the District of Columbia to be entitled to one delegate; and provided further, that it shall be the duty of the said Surgeon-General to call a conference upon the application of not less than five state or territorial boards of health, quarantine authorities, or state health officers, each of said states and territories joining in such request to be represented by one delegate.

SECTION 8. That to secure uniformity in the registration of mortality, morbidity, and vital statistics it shall be the duty of the Surgeon-General of the Public Health and Marine Hospital Service, after the annual conference required by section seven to be called, to prepare and distribute suitable and necessary forms for the collection and compilation of such statistics, and said statistics, when transmitted to the Public Health and Marine Hospital Bureau on said forms, shall be compiled and published by the Public Health and Marine Hospital Service as a part of the health reports published by said service.

SECTION 9. That the President shall from time to time prescribe rules for the conduct of the Public Health and Marine Hospital

Service. He shall also prescribe regulations respecting its internal administration and discipline, and the uniforms of its officers and employes. It shall be the duty of the Surgeon-General to transmit annually to the Secretary of the Treasury, for transmission by said Secretary to Congress, a full and complete report of the transactions of said service, including a detailed statement of receipts and disbursements.

Approved July 1, 1902.

REPORT OF THE COMMITTEE ON NATIONAL LEPER HOMES.

BY H. M. BRACKEN, M. D., CHAIRMAN, ST. PAUL, MINN.

In reporting upon the necessity for the establishment of National Homes for Lepers your committee has deemed it advisable to review the conditions existing in the various countries represented in this Association before finally making its recommendation.

Dr. F. Montizambert, a member of the committee, reports for Canada as follows:

"The only leper home in Canada is the one at Tracadie, N. B., the taking over of which by the Dominion from the Province was one of the terms of the union at the time of confederation. In this lazaretto there are at present (February 22, 1902), eleven male and seven female patients.

"There are also in New Brunswick and in Cape Breton, N. S.. three or four known cases of leprosy in the incipient stage, carefully isolated and kept under the observation of the medical superintendent of the Tracadie Lazaretto, but who have not yet been removed to that establishment. There is also what is known as a leper home on D'Arcy Island, B. C. This is not a national home. At the time of my last and unofficial visit last year there were five patients isolated on this island, all Chinamen from the Orient, and each supported by the municipality in British Columbia, in which the patient was when the disease was discovered. D'Arcy Island is within easy reach of the city of Victoria, from which supplies are sent from time to time, and the medical care of these patients is under the charge of the health officer of that city. These constitute all the known cases of leprosy in Canada. In Canada lepers are found among the people living on the eastern coast, the Icelanders living in Manitoba, and other western provinces, and the Chinese on the Pacific Coast."

Dr. Jesus Chico, a member of the committee, reports from Mexico as follows:

"The Spaniards when they first came to Mexico in 1519 found leprosy to be prevalent in Anahuac, that is, the valley of Mexico and the surrounding high plains. Hernan Cortes, moved by the sight of so many lepers, erected for their benefit a hospital which he christened "Hospital de San Lazero," in which they were isolated and cared for. Nobody at that time was able to tell when this awful disease first appeared in the country, but every well-informed native told the same story: that it was very old; beyond man's memory. Nor could any of the natives give the least idea as to its origin. In pre-Columbian times the Scandinavians who inhabited Iceland and the southern part of Greenland undoubtedly had communication with the Indians of North America, and it is known that leprosy still exists in those islands. Could leprosy have reached Mexico through these Scandinavians infecting the North American Indians, or vice versa? I think the answer must be in the negative. In either case leprosy ought to have been prevalent on the eastern coast of Canada and the United States at the time of their discovery. Of this we have no proof, for neither the French, Dutch nor English explorers give any record of this dreadful disease, which they undoubtedly would have done had they come in contact with it, for it was well known in Europe, especially after the Crusades. Is it an autochthonous complaint? I do not think so: first, because if it were so it would be dependent to a large extent on conditions as to *time* and *place* and could not be, as it is a disease of every climate, of every altitude and of people in every social rank. Shall I venture my own hypothesis as to its origin in Mexico? It must have been imported; and if so, from where? In all probability from the Sandwich Islands. We all know that the disease has prevailed to an awful extent in those islands, and the results of isolation at Molokai are now common knowledge, but it is not well known that those Kanaka people have been among the boldest navigators in the world's history: it is now generally known that the Hawaiian Islanders, paddling in their canoes, went sixty degrees southward from their own islands and settled in New Zealand, where their descendants now live under the name of the Maoris, speaking practically the same language as their ancestors. Bearing this in mind, is it not possible that the Kanaka people visited the western coast of North America? People who traveled sixty degrees south in their canoes might easily travel twenty-seven degrees northwest to the California coast. California was the cradle for the greater part of the tribes that settled in Anahuac. Reasoning thus there is little wonder that leprosy should have been found so prevalent by Cortes. It may be argued that if the South Sea Isanders came to America it should be possible to trace their descendants among the Mexican people of today. As a matter of fact, many people of the Kanaka type can be found in Mexico at the present time. I do not make much of this, for people of *many types* are to be found in Mexico at the present time.

"So much for the possible origin in Mexico.

"As already stated Cortes founded a hospital for these unfortunates on the shore of the Lake of Texcoco, not far from the City of Mexico. When I was a student this lake had receded to such an extent that the hospital was no longer on an island, but in the midst of a plain. The number of lepers had been so reduced in numbers by this plan of isolation that it was now possible to care for them in a carefully isolated ward at the hospital of San Pablo. Everything points to a decrease in the number of victims of this dread disease in Mexico. In Guanajuato, where I have the best opportunity of knowing what is going on, there certainly is a marked decrease in the number of lepers. No administrative measures are taken against their mingling with the non-leprous, but the public avoids association with them, and in consequence these people voluntarily isolate themselves. Individually I have warned the public from time to time through the local papers as to the dangers attendant upon association with the lepers or residence in a house formerly occupied by a leper. I have also applied to the proper authorities asking that some legal provision be made for the isolation of lepers, and I hope to meet with success in this matter in the near future."

As Cuba was still under the care of the United States Army when I began this report I wrote to Major W. C. Gorgas, then chief sanitary officer of Havana, for information relative to leprosy in that island. He referred me to Major J. B. Kean, then Superintendent of the Department of Charities, Havana, and through this latter official I received a very complete set of reports from Dr. Manuel J. Alfonso, Director of the San Lazero Hospital, at Havana. From these reports it appears that the asylum for lepers (Hospital de San Lazero) was founded in 1681 by a generous Jesuit, D. Pedro Alegre, a Mexican.

The time and manner of introduction of this disease into Cuba is not known. As stated in the report of Dr. Chico for Mexico, the disease was in that country at the time of the first Spanish invasion. The disease was also known to be widespread in Colombia. It could undoubtedly have been introduced into Cuba either from Mexico or from South America. It is also stated that the disease progresses very slowly and has a tendency to disappear. A record of one thousand one hundred and ninety-six cases cared for at this hospital between the years 1830 and 1900 is given as follows:

Whites	568
Blacks	389
Asiatics	199
Mestizos	40
Total	1196

It appears that in 1895 a law was "ratified" requiring the seclusion of lepers in this asylum, but that the law was not enforced until after the intervention of the United States Government. Prior to that time the lepers resided where they pleased throughout the city, and entered the hospital only when compelled so to do by their "misery." Following the United States control of Havana the police detained all lepers found in the street and placed them in the asylum. This action was often followed by insubordination. Dr. Alfonso states that this infirmity places the patients in such physicial condition "that they seem possessed of the spirit of evil." He further states that they are not friends of work, but of vice, and that among them are true criminals.

The San Lazero Hospital is a private institution and possesses considerable property, but not enough for its support. The government, while under the control of the United States, gave considerable aid to this hospital. Whether it had aid from the Spanish government, the report does not state.

Dr. Alfonso gives the following arguments in favor of leprosaria: (1) An opportunity to study doubtful cases; (2) good hygienic surroundings—plenty of bathing, fresh clothing and fresh air: all of which are important adjuncts in the treatment of this disease; (3) provision for the destruction or disinfection of all soiled clothing, bedding and dressings.

It is his opinion that after the diagnosis of leprosy is once made the infected should be compelled by law to enter a leprosarium. He contends that such a place should not be in the nature of a prison, but rather that everything possible should be done to make the isolation at such an institution endurable.

Dr. C. H. Alden, late assistant surgeon-general, reporting upon Puerto Rico, stated that there were probably a hundred cases of leprosy in that island. Under Spanish rule there had been no attempt at isolation, but under the United States Government steps had been taken to separate them from people at large, and to furnish them with proper care and treatment.

Among the new possessions of the United States is an island where many lepers have residence, but fortunately under restriction and supervision. I refer to Hawaii.

Under date of April 22, 1902, Dr. J. S. B. Pratt, executive officer of the Territorial Board of Health, informed me that there were 863 lepers at the settlement on Molokai: 519 males, and 344 females. The management of this settlement is directly under the control of the board of health.

It is the duty of the various government officials and other officials to report the probable presence of leprosy in an individual. Thereupon the suspect is sent to Honolulu to be examined by a board of five medical men, and in order to send the person to Molokai, four out of the five examiners must pronounce the case leprosy. Should three declare the case a leper and two consider it only a suspicious case, then the individual is declared a suspect and is required to report once a month to the government physician of the district in which he lives. If the disease develops later such an individual is sent again to the board of five physicians for re-examination. Specimens are taken from each case for bacteriological examination, and the result of these findings is reported to the examining board before the individual concerned is examined by that body.

The law providing for the segregation of lepers was passed January 3, 1865. It provided for the setting aside of government lands upon which to isolate such leprous persons as should be declared by the board of health liable to cause the spread of leprosy. In 1893 the law was passed setting aside the Island of Molokai for the segregation of lepers.

Provision was made that the husband or wife of a leper might be allowed to remain as a helper, or kokua, with such a leper when segregated. Dr. Pratt, in his letter already referred to, speaks of thirty-four male helpers and twenty-eight female helpers on the island. It is the duty of these helpers or kokuas to prepare the food and attend to the clothing and other things that contribute to the comfort of the lepers, whose kokuas they are.

On this island, lepers may build houses for their own use. They are also allowed to cultivate land and sell their crops. They are not required to pay rent for the land used.

Coming now to the United States proper—the leprosy commission appointed from the Marine Hospital Service, by an act of Congress approved March 2, 1899, "to investigate the origin and prevalence of leprosy in the United States," reports the presence of 278 lepers. The commission states, however, that it "cannot in the nature of things claim to have ascertained the whereabouts of every case of leprosy in the United States." It further states that "many cases are no doubt so mild as to have escaped observation altogether, and many have been purposely hidden." These statements are undoubtedly true, and without doubt some sanitary authorities have taken part in such concealment. I recall the statement of a sanitary official in one of our larger cities to the effect that there were no lepers under his jurisdiction, and that his inspectors would not dare to find one, for should they do so they knew the penalty for reporting such would be discharge from the service. Of the 278 cases reported, only seventy-two are isolated. More than half of the cases reported are American born. Other peoples are represented as follows: Norwegians 22, Icelanders 11, Swedes 8, Chinese 20, Japanese 1, Germans 12; from the Bahamas 12, Cuba 6, other West Indies 4, Mexico 3, Ireland 6, England 3, France 3, Italy 3, Spain 1. There are other foreign cases, but the nationalities of these had not been given to the commission. The commission reports that "of the states and territories, twenty-one are known to have lepers." The states that have the largest number recorded are Louisiana 155, California 24, Florida 24, Minnesota 20, North Dakota 16.

It has been the general belief that there were no American-born lepers in the northwestern states, where the population is made up largely of Scandinavians and their descendants, but this statement can no longer be maintained, for I now have knowledge of three young men, American born, who have contracted leprosy in Minnesota.

There is but one institution in the United States known as a home for lepers, and it is in Louisiana. It has had a somewhat checkered career, and has been maintained under great disadvantages. It has been well described in a recent article by Dr. Isador Dyer.

Carefully studying facts it would appear that provision for the care of lepers in Canada was an inheritance rather than a product of legislation; that the care of lepers in Mexico began with the invasion of Cortes and that the methods of caring for them had been but little if at all improved upon in that country since his time. That provision for the care of the lepers in Cuba was made at an early date by a Jesuit with philanthropic tendencies. Probably little if any improvement was made in the care of these unfortunates from the time of the establishment of the San Lazero Hospital in 1681 up to the occupation of the island by American troups. From personal knowledge I know that the life of the leper in many, if not all, of the other West India Islands is one of existence only. The action taken in Hawaii with regard to the lepers was forced upon the people before the time of American occupancy. It appears that of all the countries embraced in our Association, the United States is the only one that has made no provision for its lepers. At the same time most of the countries that pretend to take care of their leprous have little to boast of.

The commission appointed from the Marine Hospital Service recommends, one or preferably two, national leprosaria for the care of these unfortunates in the United States. It recommends the selection of sites covering broad areas in healthful localities where the lepers can have unlimited out-of-door exercise and occupation. It recommends that these homes should be made attractive and comfortable so that the unfortunate victims of this disease, instead of hiding their condition may make it known and request admission to these public institutions.

With our present knowledge of leprosy in the countries which we represent and the methods employed in its care, it seems to your committee advisable that the resolution adopted by this Association at the Indianapolis meeting in 1900 be reaffirmed, and that the work of pushing legislative action bearing upon this point be referred to our legislative committee.

The resolution referred to reads as follows:

"WHEREAS, It is a known fact that lepers are found in Canada, the United States and Mexico; that these lepers represent immigrants of many nationalities, together with some Americans; that the exclusion of leprous immigrants is impossible; that the tendency to importation of leprous immigrants in the future will be greater even than in the past; that the danger of infection of American residents abroad and the importation of the disease through these channels is greatly increased; therefore, be it

Resolved, That this Association places itself on record as favorable to the establishment of national leprosaria, which may serve not only as a refuge for lepers, but also as a home and hospital, making their lives tolerable so far as possible, furnishing employment to those who are able to work, and giving skilled medical care to all cases, with the intent of possibly curing some, and making the road to death less wearisome and painful than it now is to others."

REPORT OF COMMITTEE ON "DANGERS TO THE PUBLIC HEALTH FROM ILLUMINATING AND FUEL GAS."

By SAMUEL H. DURGIN, M. D., Chairman, Boston, Mass.

In pursuing our inquiry into the danger to health by illuminating gas, it has been the purpose of the committee to consider not only the question of its poisonous effect as observed from the ordinary clinical standpoint, and of the frequency with which the people are exposed to the effects of this agent, but also to show, from carefully conducted experiments upon human subjects, just what physiological changes are produced by long and short exposures to small amounts of the gas and to what extent the observed changes are continued or permanent.

With regard to the proposed scientific tests by which we would have been enabled to show exact results from exposure to definite amounts of the gas, I have to express disappointment in our first calculations. Had the necessary time and expense for these experiments been within such limits as were hoped for by your committee, or such as could have been assumed personally or by the Boston Health Department, the work would have been cheerfully carried out; but on careful inquiry as to the means and methods best calculated to give satisfactory results, it was found that the necessary experiments would require a much larger expenditure of money and time than we could reasonably command. Dr. Hill, Director of the Boston Health Department Laboratory, who inquired into the necessary means for this work, gave me a brief but discouraging report, from which I quote:

I visited Prof. Atwater at Middletown, Conn., and afterwards Prof. Mendel, assistant professor of physiological chemistry at Yale. I have consulted also Prof. Theodore Hough of the Massachusetts Institute of Technology. As a result of these conferences, the following conclusions are submitted:

First—The exposure of persons to illuminating gas in small amounts for short periods will give physiological changes in the blood, etc., appreciable only, if at all, by the most delicate physiological tests. In order to be of permanent value, these tests should be carried out under the most rigid conditions with attention to every detail.

The proposed use of Prof. Atwater's experiment chamber, operated with the same care as he uses in his experiments, would cost for the apparatus alone for one week, about $500, besides the professional fees to the trained experts who would be necessary to run the machine and those required to make the chemical and physiological tests, amounting probably to $1,000. In the course of the week, the effect of the gas could only be tested on one person because it would require at least two days to determine the patient's condition in the first place (Prof. Atwater spends four days on this "calibration" in his work), then say three days for exposure to the gas, and say two days more to determine the conditions after the gas is no longer administered.

The organization of such an experiment will be obviously both difficult and time-consuming. Very many delicate physiological tests which the physiologists and chemists have not at their finger ends must be looked up and put on a working basis, apparatus must be fitted up, for some purposes, new apparatus must be devised. The chemical analysis of the gas must be provided for, etc., etc. To prepare properly for the chemical tests of the blood and urine alone, Prof. Mendel thinks it would be necessary to put a man into training for several months before the experiments themselves.

Second—The trained physiologists consulted, hope for very little definite results from a single experiment of this nature, however carefully conducted. Prof. Atwater has been at work six years on two problems only in the normal man. His definite experiments have cost him about $400 each. Even so, he says he has obtained but a glimmering into the subject. Prof. Mendel thinks that a regular commission with plenty of money and two or three years expenditure of time would alone suffice. I give these views to show how large a problem is involved.

Third—If we abandon this elaborate investigation and attempt to do a few experiments in much cruder shape, it is a question whether we could decide the point which you wish decided. Exposure to amounts of gas sufficient to produce headache, could not be well carried on for very long, for the patients would object. Exposure to amounts of gas less than this, would not in all probability be of decisive value unless carried on for long periods. It would of course be difficult to find anyone willing to submit to such restraint but if that difficulty be overcome, there still remains the difficulty of finding any expert with sufficient training in these lines to detect by physiological and chemical methods, the minute effects of the exposure.

I did not appreciate either the complication of the problems to be solved or the time and money required until I began to look the matter up in detail and to consult physiologists and chemists about it."

The experimental work was then abandoned, but with the hope that it might be taken up by some well equipped department of the government, perhaps the newly established Public Health Service at Washington.

The frequency with which persons are innocently exposed to this dangerous agent from insecure gas pipes and fixtures, was investigated in Boston in 1897, and the results reported in a short paper on the subject by the reader at our meeting in Ottawa in 1898.

It was shown in this investigation that the common condition of gas pipes and fixtures was very poor, the moderate small leaks very numerous, and that a moderate increase of pressure in the fixtures above the normal gas pressure, produced leakage in eighty-nine percent of all the houses examined. It was also shown that the number of fatalities from illuminating gas was not only large but was increasing from year to year.

In order to ascertain to what extent the medical profession in Boston had recognized cases of acute and chronic poisoning by illuminating gas, I sent out letters to twenty-two hundred physicians, asking for their personal experience in such cases.

I received replies from 460 physicians.

Two hundred and forty-six of them reported no experience.

The other 214 reported 1,025—("many," "small number," 'some" "several") cases of acute poisoning.

Three hundred and seventy-four of these resulted fatally.

Two hundred and eighty-eight were found dead and eighty-six lived from one hour to twelve months.

Six hundred and twenty-three made complete recovery in periods varying from a few hours to three years.

Twenty-eight made partial recovery, while under observations of from three weeks to twenty-one months.

The following symptoms are quoted from the physicians reports: Nausea, Headache, Vomiting, Malaise, Dizziness, Tired, Loss Vitality, Debility, Vertigo, Cyanosis, Convulsions, Coma, Semi-coma, Unconsciousness, Semi-unconsciousness, Drowsiness, Lassitude, Weakness, Fainting, Depression, Prostration, Exhaustion, Anemia, Anorexia, Shallow Resp., Difficult Resp., Rapid Resp., Laboured, Resp., Irregular Resp., Rapid Pulse, Weak Pulse, Irregular Pulse, Ringing in Ears, Blurred Vision, Subnormal Temperature, Muscular Pain, Deafness, Cardiac Irregular, Irritable Heart, Pulmonary Irritation, Faulty Circulation, Bronchitis, Neurasthenia, Delirium, Cough, Hysteria, Nervousness, Hemetemesis, Vaso-motor Disturbance, Neuritis, Diabetes, Loss Memory, Indigestion, Pain in Back, Sore Throat, Cold Extremities.

One hundred and fifty-four cases of chronic poisoning were reported as resulting from continued exposure to small amounts of gas from leaking fixtures.

The symptoms reported in the chronic cases were practically the same as those reported in the acute cases and need not be repeated in this connection.

A single experiment made by Dr. Hill upon himself, and one recent case in detail reported by Dr. Carson, Port Physician of Boston, may be of interest:

CASE OF DR. HILL.

"Closet used, containing 200 cubic feet.

Dr. Hill entered closet at 11:25, pulse 92, respirations 20. Small amount of gas admitted, says ½ percent of cubic contents of room. No effect in thirty minutes.

Then gas was turned on from tap directly and allowed to flow freely into the closet for thirty minutes. Dr. Hill still in closet with door pasted up.

No effect noticeable for twenty-five minutes except gassy taste and smell. Then pulse rose to 106, respirations remained 20, headache. After leaving gas chamber, felt shaky and dizzy, headachey. A very little exercise (putting on overcoat to go out) resulted in rapid heart beats, 120, and short breath for an hour. Heavy feeling, headache, very slight dizziness and general useless feeling lasted all afternoon. Headache began after supper and continued till bedtime."

CASE REPORTED BY DR. CARSON

occurred October 21, 1902, and reported November 19, 1902.

"Full recovery has not yet taken place though the only symptoms at present are general weakness and a secondary anemia.

When first seen patient was unconscious, breathing sterterous; radial pulse not felt; pulsation of heart 140, weak and irregular; extremities very cold; axillary temperature 101 3-5 F. Considerable œdema of face, eyelids and hands; cheeks cherry red in color; mucous membrane of mouth cyanotic; beneath finger nails same condition. Patient was unconscious for fully two hours after being found; after regaining consciousness complained of severe headache and extreme prostration. A peculiar gassy odor of the breath continued for about twelve hours. Patient vomited several times while unconscious and once or twice after. First examination of urine showed slight trace of albumen; sediment showed few granular casts and a little blood. Sugar negative. Examination of urine one week later, negative.

BLOOD COUNT.

October 22.—White corpuscles 17,500. Reds not counted. Hæmaglobin 70 percent.

October 29.—White corpuscles 16,000. Reds not counted. Hæmaglobin 70 percent.

November 19.—White corpuscles 12,400. Reds 4,960,000. Hæmaglobin 75 percent.

Differential count about normal."

In view of the well-known fact that illuminating gas contains from 7 to 30 percent of carbonic oxide, one of the most deadly poisons which could be liberated into an atmosphere for respiration; that in small quantities it may be tolerated or misapprehended by the sufferers for a sufficient length of time to produce headache, nausea, malaise, anemia, dizziness and a long list of concomitant symptoms, all of which disappear with the removal of the gas or patient, that in larger quantities this agent is most active and even popular in the destruction of human life, that the gas pipes, fixtures and fittings are found to be as faulty in material and workmanship as are plumbing and plumber's materials, with results far more serious, it seems to your committee reasonable to accept the strong evidence against this poisonous agent, to recognize the ease with which it becomes a part of the atmosphere of the home, and to recommend that gas fitters and gas fitters' materials be placed under municipal control and regulation as is now the case with plumbers and plumbers' materials in many of the cities of this country.

I have subjoined for your inspection an Act of the Massachusetts Legislature of 1897, for the City of Boston and with it a copy of the regulations made and in force in our city.

GAS FITTING.

(Chap. 265, Acts of 1897.)

AN ACT RELATIVE TO THE LICENSING OF GAS FITTERS AND TO THE SUPERVISION OF THE BUSINESS OF GAS FITTING IN THE CITY OF BOSTON.

Be it enacted, etc., as follows:

SECTION 1. No person, firm or corporation shall engage in or work at the business of gas fitting in the city of Boston after the first day of October in the year eighteen hundred and ninety-seven, either as employer or as a journeyman, unless such person, firm or corporation has received a license therefor in accordance with the provisions of this act. The word "journeyman," as used in this act, shall be deemed to mean one who personally does any gas fitting, or any work in connection therewith which would be subject to inspection under the provisions of this act.

SECT. 2. Every person, firm or corporation desiring to engage in the business of gas fitting in the city of Boston shall make application therefor to the building commissioner, and shall, at such time and place as may be designated by the board of examiners hereinafter provided for, to whom such application shall be referred, be examined as to his qualifications for such business.

SECT. 3. The board of examiners shall consist of the building commissioner, the chairman of the board of health, who shall be *ex officiis* members of said board and serve without compensation, and a third member, to be chosen by the board of health, who shall be a practical gas fitter of at least five years' continued practical experience during the years next preceding the date of appointment. Said third member shall be chosen within thirty days after the passage of this act, for a term ending on the first day of May in the year eighteen hundred and ninety-eight, and thereafter annually; and he shall be allowed a sum not exceeding five dollars for each day of actual service, to be paid from the treasury of the city of Boston.

SECT. 4. Said board of examiners shall, as soon as may be after the appointment of said third member, meet and organize by the selection of a chairman and clerk, and shall then designate the times and places for the examination of all applicants desiring to engage in or work at the business of gas fitting in the city of Boston. Said board shall examine said applicants as to their practical knowledge of gas fitting, shall submit the applicant to some satisfactory form of practical test, and, if satisfied of the competency of the applicant, shall so certify to the building commissioner, who shall thereupon issue a license to such applicant, authorizing him to engage in or work at the business of gas fitting, first requiring him to register in the office of the said building commissioner his name, place of business or residence, license number, date of examination, and in what capacity licensed. In case of a firm or corporation, the examination of one member of the firm, or of the manager of the corporation, shall satisfy the requirements of this act. The fee for the license of any employing gas fitter shall be two dollars, and for a journeyman, fifty cents; and said license shall continue in force until revoked or cancelled, but shall not be transferable.

SECT. 5. The building commissioner shall appoint, after the first day of October in the year eighteen hundred and ninety-seven, such a number of inspectors of gas fitting as the board of examiners may from time to time determine. Said inspectors shall be practical gas fitters of at least five years' continued practical experience, and shall, before appointment, be subject to an examination before the civil service commissioners. The compensation of said inspectors shall be determined by the building commissioner, subject to the approval of the mayor; and such inspectors shall hold office until removed by said commissioner, with the approval of the mayor, for malfeasance, incapacity or neglect of duty. Said inspectors shall inspect all new work relating to gas fitting in new and in old buildings, the connections and use of such work, and shall report all violations of this act or of any act or ordinance relating thereto, which now exists or may be hereafter enacted or ordained; and they shall also perform such other appropriate duties as may be required by the building commissioner.

SECT. 6. Every licensed gas fitter shall display his license number conspicuously at his place of business.

SECT. 7. On and after the first day of October in the year eighteen hundred and ninety-seven no building shall be piped or fitted for gas nor shall any repairs be made in such piping or fittings, nor fixtures placed, unless a permit shall be granted therefor by the building commissioner. Every licensed gas fitter desiring to perform any work relating to piping or fitting a building for gas, or to repair gas piping or fittings, or to place fixtures therein, shall file an application therefor at the office of the building commissioner, giving the correct location, name and address of the owner, the intended use and material of the building, and a full and complete statement of the work proposed and material to be used, and shall, if required by said building commissioner, furnish a plan thereof, which shall be subject to his approval. All materials used and work performed under the provisions of this section shall be subject to such regulations as shall be made by the board of health and the building commissioner.

SECT. 8. No gas pipe which may be introduced into any building shall be let into the timbers, beams or girders, unless the same is placed within thirty-six inches of the end of said timber, beam or girder, and in no building shall the said pipes be let into the timbers, beams or girders, more than two inches in depth. No person shall disconnect or remove any gas meter, except the duly authorized representative of the gas company owning such meter.

SECT. 9. All gas brackets shall be placed at least three feet below any ceiling or woodwork, unless the same is properly protected by a shield, in which case the distance shall not be less than eighteen inches. Nothing in this act shall be construed to affect the operations of any gas company upon its own premises or upon its mains and surface-pipes.

SECT. 10. The board of health of said city by its inspectors shall from time to time, as it deems proper, inspect the gas fixtures and appliances in any building and shall make such requirements relating thereto as it deems the public health requires, and the owner of such building shall comply with such requirements.

SECT. 11. Any person violating any of the provisions of this act shall be deemed guilty of a misdemeanor, and shall be subject to a fine of not exceeding one hundred dollars for each offense, and if such person has received a license under this act his license may be revoked by the building commissioner.

SECT. 12. The building commissioner shall include in his annual report to the city council a report of the proceedings of the building department under this act, and shall include therein a report of the board of examiners appointed under this act, giving their proceedings during the year ending on the first day of February.

SECT. 13. All acts and parts of acts inconsistent herewith are hereby repealed.

SECT. 14. This act shall take effect upon its passage, except so far as is hereinbefore otherwise provided. (*Approved April 10, 1897.*)

REVISED REGULATIONS

PERTAINING TO GAS FITTING AND GAS FITTING MATERIALS ADOPTED JULY 29, 1898, BY THE BOARD OF HEALTH AND BUILDING COMMISSIONER, TO TAKE EFFECT OCTOBER 1, 1898.

(Authorized by Chapter 265, Acts of 1897.)

SECTION. 1. In all cases of repair of leaks, a notice giving the location and extent of all work performed shall be filed with the building commissioner immediately upon completion of the same.

SECT. 2. No pipe or fitting shall be covered or concealed from view until approved by one of the gas inspectors of the building department, or for twenty-four hours after notice has been given to the building commissioner.

SECT. 3. No pipe shall be so laid as to support any weight (except fixtures), or be subjected to any strain.

SECT. 4. All outlets for fixtures shall be securely fastened to the satisfaction of the building commissioner, all outlets not covered by fixtures shall be left capped, and the number of burners for each outlet shall be marked on the builders' plans.

SECT. 5. Any pipe laid in a cold or damp place shall be properly dripped and protected.

SECT. 6. All swing brackets shall have a globe or guard to prevent its burner from coming in contact with the wall. All bracket outlets shall be at least two inches from window or door casings.

SECT. 7. Gas or combination fixtures in all public buildings, theaters and public halls shall be made safe to the satisfaction of the building commissioner.

SECT. 8. All stop-pins to keys or cocks or fixtures shall be screwed into place.

SECT. 9. The use of gas fitters' cement is prohibited, except in putting fixtures together.

SECT. 10. Gas shall not be let on in any building until the work performed has been approved by the building commissioner. Inside services shall be tested by the fitter who receives the permit to connect the service or meter.

SECT. 11. There shall be a brass straightway valve on the service-pipe close to the foundation wall, one at the inlet and one at the outlet side of each meter. Iron valves shall not be used.

SECT. 12. There shall be a final test, by a gas fitter, of all fixtures and pipes by two inches of mercury, which must stand five minutes; this test to be made in the presence of one of the gas inspectors of the building department; the gauge to be made of glass tubing of uniform interior diameter, and so constructed that both surfaces of the mercury will be exposed.

SECT. 13. All gas-pipe shall be of wrought-iron, all fittings of malleable iron, and all meter connections of lead pipe of the same size as the fit or riser. Galvanized fittings are prohibited.

SECT. 14. Brass solder nipples shall be used on all meter connections.

SECT. 15. No riser shall be left more than five feet away from the front foundation wall.

SECT. 16. All buildings shall be piped according to the following scale and properly fastened:

Iron pipe,	⅜ inch,	26 feet,	3 burners.
“	½ “	30 “	6 “
“	¾ “	50 “	20 “
“	1 “	70 “	35 ”
“	1¼ inches	100 “	60 “
“	1½ “	150 “	100 “
“	2 “	200 “	200 “
“	2½ “	300 “	300 “
“	3 “	450 “	450 “
“	3½ “	500 “	600 “
“	4 “	600 “	750 “

When brass piping is used on the outside of plastering or woodwork, it shall be classed as fixture.

SECT. 17. All outlets and risers shall be left capped or covered with fixtures.

SECT. 18. All service-pipes in cold or damp places shall be painted with two coats of red lead and boiled oil.

SECT. 19. Gas outlets for burners shall not be placed under tanks, back of doors, or within four feet of any meter.

SECT. 20. All gas-burners less than three feet from ceiling or woodwork shall be protected by a shield.

SECT. 21. All brass tubing used for arms or stems of fixtures shall be at least No. 18 standard gauge and full size outside so as to cut a full thread. All threads on brass pipes shall screw in at least 5-16 of an inch. All rope or square tubing shall be brazed or soldered into fittings and distributers, or have a nipple brazed into the tubing.

SECT. 22. All cast fittings such as cocks, swing joints, double centres and nozzles shall be *extra heavy*. The plugs of all cocks must be ground to a smooth and true surface for their entire length, be free from sand-holes, have not less than ¾-inch bearing (except in cases of special design), have two flat sides on the end for the washer and have two nuts instead of a tail-screw.

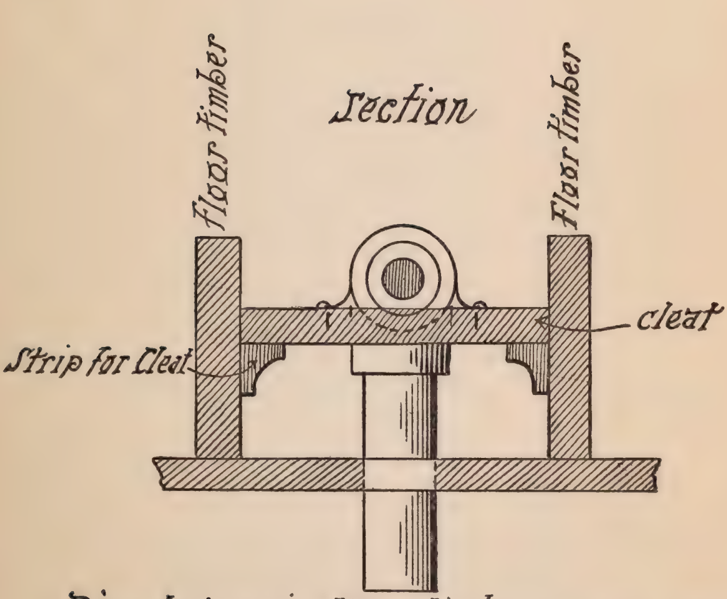
SECT. 23. Outlets for gas ranges shall have a diameter not less than that required for six burners, and all gas ranges and heaters shall have a straightway valve on the service-pipe.

SECT. 24. Pipes in buildings shall be laid above timbers instead of beneath them, where it is possible to do so.

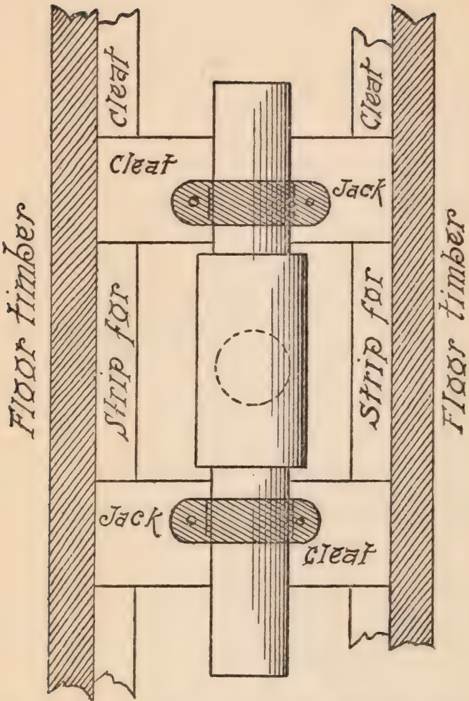
SECT. 25. No second-hand gas pipe shall be put into use in any building without the written permit of the building commissioner.

SECT. 26. Drops or outlets less than ¾ of an inch in diameter shall not be left more than ¾ of an inch below plastering, center piece or woodwork, and other outlets shall not project more than ¾ of an inch beyond plastering or woodwork.

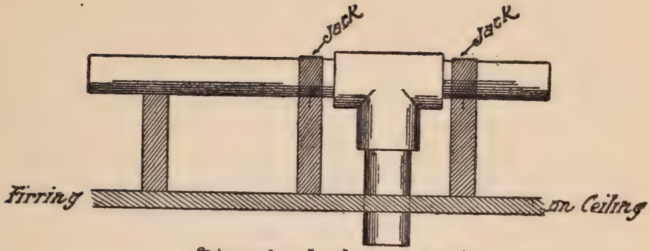
SECT. 27. Fastening boards shall not be cut away to accommodate electric wires. All outlets shall be fastened according to the following diagrams:



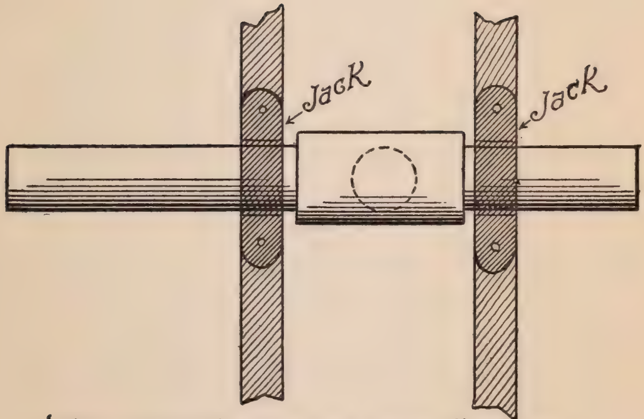
Pipe between floor timbers, pipe running in same direction.



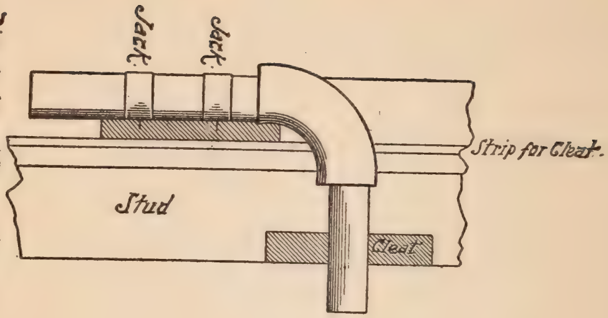
Plan looking down on pipe running in same direction as floor timbers.



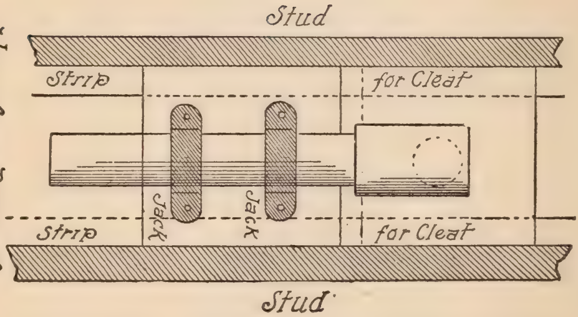
Pipe over timbers Section.



Plan showing pipe over timbers
-looking down-

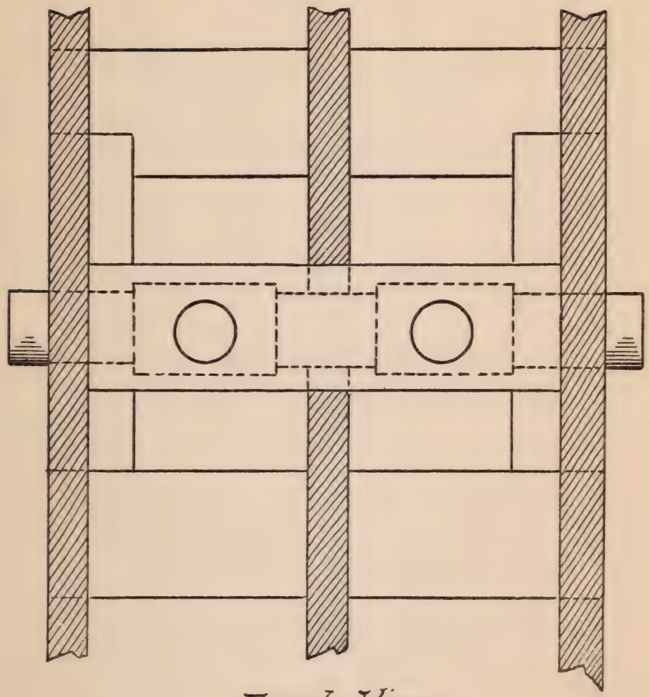


Pipe in Partition. Section

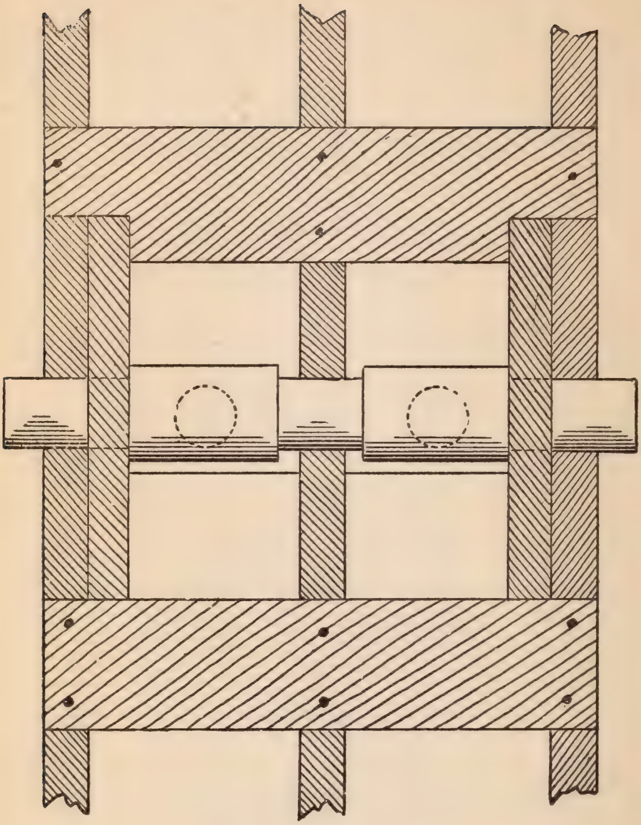


View from Back of Partition

Gas Pipe in Breastwork.



Front View.



Back View.

SECT. 28. All iron pipes used for piping buildings, all arms, and all items of fixtures, shall be of the kind classed as standard pipe, and shall weigh according to the following table:

Size of Pipe.	Pounds Per Foot.
⅛-inch pipe	.24
¼-inch pipe	.42
⅜-inch pipe	.56
½-inch pipe	.85
¾-inch pipe	1.12
1 -inch pipe	1.67
1¼-inch pipe	2.24
1½-inch pipe	2.68
2 -inch pipe	3.61
2½-inch pipe	5.74
3 -inch pipe	7.54
3½-inch pipe	9.00
4 -inch pipe	10.66

SECT. 29. No gas pipe shall be laid in cement, unless the pipe and channel in which it is placed are covered with tar, nor within 6 inches of an electric wire.

SECT. 30. Wherever spark-lighting or self-lighting burners are used the mercury test shall be applied to the cocks.

AMENDMENTS.

AUGUST 16, 1899.

At a meeting of the Board of Health and Building Commissioner this day, it was voted to amend the revised regulations pertaining to gas-fitting materials adopted July 29, 1898, as follows:

In Section 22 strike out in the second and third lines the words "extra heavy" and insert in place thereof the following: "Standard fittings, except for factory use, where extra heavy or mill fittings shall be used."

Also in Section 22 strike out in the sixth line "except in cases of special design" and insert after the word "bearing" the following: "On all cast fittings and 11-16 of an inch on all turned fittings."

All stems of fixtures of two lights or more each, shall be not less than ¼ of an inch iron-pipe size. L-burner cocks shall not be used at the end of chandelier arms.

A lithograph print of designs will be issued to show the outline of standard and extra heavy fittings and chandelier cocks, and the weight of each fitting.

REPORT OF COMMITTEE ON THE RELATIVE IMMUNIZING VALUE OF HUMAN AND BOVINE VACCINE VIRUS.

GENTLEMEN: In 1798 Jenner published his "Enquiry Into the Causes and Effects of Variolæ Vaccinæ." Now, though it is an historical, as well as a modern scientific fact, that vaccination has protected and will protect today against smallpox, it nevertheless becomes incumbent upon us to precisely understand just what Jenner meant by vaccination, and what has been implied in the term by all the great teachers and practicers of the art during the past century, and what we understand by it today. Without going greatly into detail it may be said that what Jenner did, and what he taught, was to introduce by inoculation a disease, which causes a vesicle of a particular character on the teats of a cow or on the tender skin of the belly or flanks of a calf, the virus of which when inoculated into a person produces a vesicle of a similar character. The vesicle can be produced through inoculating several calves in a series with virus from a smallpox patient, and this smallpox virus thus modified produces in other calves, in man and monkeys a disease, vaccinia, which protects against smallpox completely, in practically all cases for ten years, in a great percentage for twenty years, and which, though the protection decreases with the distance of years from primary vaccination, yet always produces a marked effect in lessening the virulence of smallpox, should a person once vaccinated take the disease.

The experiments of Pearson, Tanner and Barry had fully proved by 1801 Jenner's teaching that cowpox and grease or true horsepox, were sources of a vaccine, protecting against smallpox; while it is of much practical interest to remember that Drs. Pearson and Woodville, of London, physicians to the smallpox hospital, obtained their first lymph in 1799, not from Jenner, but from an outbreak of cowpox in a dairy in Grey's Inn and from another at Mary-le-bone. We are informed that these as well as Jenner often went back to the cow for fresh lymph. These early experimenters were anxious to determine all the problems, connected with vaccine from different sources; whether all were the same or different and also whether they were altered by continuous transmission from arm to arm. It is related that Woodville's first results were milder than those of Jenner, who wrote of the inflammatory results as regards the axillary glands and the formation of an ulcer on the pock. The efficacy of a lymph in those early days was also tried by the variolous test. Willan writing in 1806 mentions that from personal inquiries amongst eighty-six medical men whom he had met during the course of the previous three months, he had been made aware of over 18,000 vaccinations done by them, all the cases having remained free from smallpox, though most of them were either inoculated with variolous matter or exposed at different periods to contagion. At the same time it was no uncommon thing for parents to hesitate between the old practice and the new, to have some of their children vaccinated and some variolated; and Willan states that Wachsel, of the smallpox hospital, had noted this in more than a hundred families, without observing an instance of variolous fever and eruption among vaccinated children. Abundant evidence was soon obtained from every country to which the marvelous discovery rapidly spread, of the protective effects of vaccination against direct inoculation as well as exposure to smallpox. Published plates by Jenner, Aiken, Bell and others show vaccinia one hundred years ago exactly as we know it at the present day.

Some years after Jenner's death a new spirit of inquiry arose regarding cowpox as the source of vaccine lymph, many believing that the vesicles being produced in vaccination were not comparable with those of early times, and that the protection afforded by vaccination was not so great. These experiments were important in showing the need of taking lymph from the vesicles before they had broken and had been subjected to secondary pus infection, thus corroborating Jenner's opinion: "That the most material indisposition, or at least that which is felt most sensibly, *does not come primarily from the first action of the virus on the constitution*, but that it often comes on *if the pustules be left to chance as a secondary disease;*" and again he says, "I am more and more convinced of the extreme mildness of the symptoms merely *from primary action* of the virus in the constitution."

As we read these words of that marvelous old-time experimenter and clinician, which, in these days glorious with the victories of aseptic surgery, could not be improved upon in describing the vaccine vesicle previous to secondary infection, we are forced to use the words of Horace: "*Justum et tenacem propositi virum.*"

The accuracy and comprehensiveness of the work done by Jenner and his contemporaries is best measured by the fact that not until within the last twenty years of the century, when bateriology had begun to take its place as the most important application of experimental science to medicine, did the real nature of vaccine become a matter of renewed investigation. With the appearance of an epidemic of smallpox in 1870 of world-wide extent and of unexampled severity, vaccination was made compulsory in England and most European countries; vaccination stations and regularly appointed public vaccinators became a routine measure and the epidemic was stayed. The disease reappeared in many quarters in 1881, but during the interval, in England especially, a certain amount of agitation against the compulsory operation of the Act had developed, the chief reason being that certain diseases, as syphilis, were asserted to have been introduced with the vaccine. As an outcome of this, Dr. Robert Cory submitted himself to direct inoculation with lymph taken from syphilitic children, and on four different occasions, once in 1878, in 1879, in May, 1881, and in July, 1881, and in the last experiment was inoculated with the disease. The conclusion arrived at was that the disease was to a limited extent capable of being transmitted when the virus is taken from a vesicle in a child suffering from the disease in its primary stage. It is not to be forgotten, however, that the syphilitic disease in these vaccinated children was in so acute a stage that no physician would have thought of utilizing them as a source of vaccine lymph for the ordinary purposes of arm-to-arm vaccination.

It was, however, as a result of these discussions and experiments that the question of returning to the cow as a source of vaccine lymph came into prominence. In the 1883 Report of the Local Government Board, is described the vaccine station of the Board, as being situated at 95 Lamb's Conduit street, London, on the ground floor of the house.

The then practice was to vaccinate a calf on Saturday with lymph stored in tubes. This calf supplied lymph on the following Thursday to (1) vaccinate children brought to the station; (2) supply of lymph to the national vaccine establishment; (3) the direct vaccination of another calf; (4) the vaccination from tube of another calf the next Saturday; (5) the calf vaccinated on Thursday supplied lymph for vaccinating children on the following Tuesday, and (6) for supplying tubes for the national vaccine establishment.

Such is the beginning of the supply of bovine lymph in England, and how small this beginning, is gathered from the Reports, which show that from the Animal Vaccine Station of the Local Government Board there were sent out:

	1883.	1894.
Charged ivory points	9,249	13,139
Tubes (capillary)	2,151	114
Number of applications	1,666	1,770

Thus we see that at most the number per application did not exceed ten points; and it is quite clear that from two calves a week, lymph absolutely fresh, would always be on hand to supply such small orders.

Dr. Cory states in the report that with perfectly fresh lymph 990 primary vaccinations in 1000 were successful; and with perfectly fresh lymph, human or bovine, 99 primaries were successful at the National Vaccine Stations. The report of the British Army Medical Department of 1882 showed that only 40 per cent. of primary vaccinations were thoroughly successful with stored lymph, mostly all of which was humanized lymph in tubes, owing doubtless to inert lymph due to age or imperfect storage.

While in a very remarkable manner, the routine of 1883 is found continued in the report of the Local Government Board for 1900, and while the station is situated in the same place, the extension of the work may be guaged by the fact that although there was almost no smallpox in the United Kingdom in 1900, vaccine was supplied to the extent of 444,221 charged capillary tubes of glycerinated vaccine, sent in response to 50,512 applications from public vaccinators, while there are in addition to the animal vaccine station 22 other stations throughout the kingdom, known as educational vaccination stations.

The Report states further that 155 calves were vaccinated during the year, having a total weight on reception at the station of 43,964 pounds, and on dismissal 45,301, or an average gain of 8.62. Of these 142 were vaccinated direct from other calves and thirteen from stored lymph. Insertions to the number of 6,697 in calf-to-calf operations produced 6,521 vesicles. In those vaccinated with lymph stored in tubes of 513 insertions 440 gave vesicles, or in the two methods a percentage insertion success of 97.37 and 85.77 per cent. respectively. No material difference resulted whether the lymph was taken 96 or 120 hours after vaccination.

Of primary vaccinations done at the station on children there were 1,892, five insertions of the lymph being made in each case. All but thirteen succeeded at the first attempt. There were 374 from calf to arm. Of these all returned for inspection but three. Of the 371 examined, 347 had taken in five places, nineteen in four, three in three, one in two and one failed on first attempt. Thus the insertion rate was 98.22 per cent. The remaining primary vaccinations were done with stored glycerinated lymph. There were eleven cases brought back after first inspection on account of some abnormal course of their vaccination. In most the abnormality consisted of "sore arm," caused, in most cases, by *domestic maltreatment*.

Dr. Frank Blaxall, an old officer of the Local Government Board, reports on the glycerinated calf lymph establishment, with stables in Little James street, new and extended, and the laboratory at the

Jenner Institute of Preventive Medicine. The results of the reports on the 420,425 cases vaccinated with glycerinated lymph and reported upon show 97.7 per cent. of case success and 91.2 per cent. of insertion success. The lymph was from 362 calves. All were examined at slaughter by a veterinary surgeon and declared to be healthy. Four other calves, apparently healthy, were on examination found slightly tubercular and the lymph taken from them was destroyed.

These statistical results are of much importance since they indicate that, as these Government Reports have shown from year to year, the production of normal vaccine, is capable of being accurately carried out, since successive reports have told the same story. Similarly these results show that vaccine taken 120 hours after inoculation always shows the presence ot a non-sporing series of bacteria, known as "staphylococci." These are such as are found on the skin of men and animals, in the hair follicles and sweat glands, and when so found do not possess any specific pathological significance when injected into rabbits or guinea pigs, after being grown in bouillon. Practically the only other form found in lymph is a spore-bearing form (*B. Mesentericus*) from the bowel contents of the animals. Although as seen in the report quoted, the use of lymph directly from the calf to the arm is not shown to have any but the best results, yet in view of the fact of the number of such bacterial forms possibly present in the vaccine vesicle where some carelessness in handling animals may have existed, it has become after ten years' experience the practice to send out vaccine after a month's treatment in glycerine by which the extraneous organisms are killed. The lymph taken from a series of animals inoculated from the same stock lymph is found to vary both in quality and amount. Report after report of the National Vaccine Establishment in London refers to the common experience that in any considerable number of calves all seemingly healthy, some do not take and are said to be "out of condition," while others do not produce vesicles to the same extent or supply the same amount or quality of lymph. A very extended physiological study of twenty-five calves by Dr. Alan B. Greene, of the Jenner Institute, has failed to find any such distinctive blood conditions as would explain why fourteen of these yielded vesicles of good quality, while the remaining six gave vesicles of poor quality and five none at all.

The same experience is obtained at vaccine farms everywhere, on this continent, and indeed, those in the most southerly localities find that their calves fail largely to respond in periods of continued hot weather. These facts are wholly in keeping with the differences which are known to occur in the severity of smallpox in different individuals even of the same family where exposed, though unvaccinated, to a case of smallpox, or indeed to almost any infectious disease; and also illustrate how vaccine produces a more severe reaction in one person than another, quite apart from the secondary effects or pustulation.

Our studies have therefore brought us to several quite obvious conclusions regarding vaccinia. Summed up briefly these are:

First—That it is variola modified by being transmitted in either some accidental or experimental manner from man, its normal host, to some animal of the bovine species.

Second—That in common with other microbic diseases, its germ has an optimum temperature at which it grows best and it is logical to conclude, that since the cow's normal temperature is 101½ to 102½ degrees F., the germ of smallpox is modified in virulence, by its successive passages.

Third—Many experiments go to show that the same vaccine lymph not only produces different results in a series of animals, but also that the lymph taken from such being different in quality may so depart still farther from the normal in subsequent transmissions, till it may fail entirely to "take" or may produce an imperfect vesicle and be but little protection either against a subsequent vaccination with a stronger lymph or against smallpox.

Fourth—That in these respects it acts in a manner similar to smallpox, which in different outbreaks shows marked variations in virulence, and affects very variously different individuals infected from the same source.

Fifth—That an experimental testing of every batch of vaccine before sending out from a station is a logical necessity, growing out of such variations; and this can readily be done on children and by comparative tests on other calves and guinea pigs.

How vaccination immunizes against smallpox or against a subsequent inoculation with vaccine has long been a question of much interest; but it is only within recent years that an adequate explanation of the fact has been offered. Since 1894 the production of the antitoxin of diphtheria through the injection into horses of gradually increasing doses of the toxine or poison produced by the growth of the diphtheria germ, has supplied us with a theory, which explains how the introduction of vaccine or the modified smallpox germ sets up either through using up those substances in the blood and tissues which supply food, or causes, through the formation of some new *anti-body*, a condition inimical to the virus when introduced subsequently.

The latter theory is that now generally held and is proved by the neutralizing of the infective qualities of vaccine, through adding to it in test tubes, the filtered serum of the blood of calves which already have been vaccinated, or that of men or monkeys which have suffered from smallpox, just in the same way as the serum from immunized horses can neutralize the virulence of the toxine filtered from the cultivated germs of diphtheria.

An extended series of experiments, carried on in 1898, by Béclere, Chambon and Menard, directors at the Institute of Animal Vaccine, Paris, have been of the greatest importance in showing the gradual increase of the immunity caused by vaccination. The procedure was simply to inoculate by a syringe, calves, subcutaneously with fresh glycerined lymph, and after 3, 4, 5, 6 and 7 days, inoculate the animal on the skin in the usual manner and watch the progress of the vesicle. The effect on the vaccine vesicle of such subsequent inoculations, for instance, was: (*a*) Vesicles appearing a day sooner than in normal vaccination; (*b*) Vesicles modified in external appearance, rapidly arrested or aborted in their development; (*c*) lymph with almost no virulence four days after inoculation .

It is interesting to note that experiments on guinea pigs gave similar results. The results of these experiments with the serum from vaccinated calves and men and monkeys which have suffered from smallpox, summed up were:

First—The serum of a heifer taken fourteen days after inoculation is endued with immunizing powers preventive and curative, but also produces, when mixed with fresh vaccine, an anti-virulent lymph.

Second—Vaccination with such lymph produces this effect whether the vaccine be introduced into the blood, the skin or deeper tissues.

Third—The serum from smallpox convalescents exerts the same anti-virulent action on vaccine as does that from variolized animals, as monkeys.

Fifth—The period of vaccinal immunity varies much with different species, but the anti-virulent property of blood goes on decreasing and may disappear completely, although the skin in the same person may still resist re-inoculation with vaccine.

Sixth—This anti-virulent property varies in different persons, but persists for twenty, thirty and even fifty years or over, in its partial protection against variolous and vaccinal infection.

Seventh—We do not yet know whether this substance acts directly on the infected agents as a virulicide or whether it acts as a stimulant on the tissue cells of the organism.

We have dealt at some length with the principles and practice underlying the preparation of both humanized and bovine vaccine, and have explained the theories of its immunizing power based upon experiment. We may now very briefly refer to some of those occasional aberrations from the normal in vaccination and complications attendant upon the presence of the vaccine disease in the system.

Probably such variations can be best understood by recalling the picture of smallpox as it appears in different persons and in different epidemics. Van Swieten, the great Dutch physician of the eighteenth century, described in 1759 a mitigated type of the disease which appeared in epidemic form in that country. He states: "The primary fever is often little more than a febricula, and the pustules seldom exceed more than from one to two hundred. The form is so mild that secondary fever is not manifested and constantly is wanting, convalescence coming on the eighth day of the eruption."

Who that has had experience in the epidemic of the past three years could not have used these exact words to describe cases within his own experience? On the other hand, instead of this, we have in severer smallpox, as Prof. Jaccoud describes, a true ulcerative and suppurative dermatitis, involving, as every one knows, who has seen severe smallpox, the whole body, but especially the tissues of the face, often so completely as to change it into one suppurating sore with the features distorted out of all semblance to the human.

Now, if we were not in a position to accurately control the operation and forestall in practice, secondary infections in vaccination, we might expect to find, owing to the difference in the susceptibility of persons, variations in the severity of the disease, perhaps relatively as great as between different types of smallpox. Fortunately we have in these modern days, thanks to the progress of science in the production of vaccine lymph, all except the *individual* element completely under our control.

We have seen what the practice in the National Vaccine Establishment in London is, and I am personally able to say from observation that in the great laboratories in the United States, no expense has been spared to provide every possible means by which vaccine lymphs of a normal virulence, free from extraneous germs, are today prepared with a care, I believe, not exceeded in any government laboratory in the world. Why then do we hear complaints, especially from the profession, of abnormal results in vaccination? I shall give the reasons as they appear to me in their order:

First—A general absence of any teaching, in our schools of medicine, of the principles and practice of vaccination, as understood and carried out by Jenner and all his true successors, and as it is practiced today in Germany, France and England.

Second—A very general absence amongst the younger members of the profession and wholly so in the younger generation of the people, of a knowledge of what a case, and yet more an epidemic of smallpox of normal virulence means.

Third—A very general impression, till recently, in both these classes, that vaccination could be reduced to a mere name, and that it did not necessarily mean the communication of a true disease in order to immunize against smallpox.

Fourth—A similar impression for a time in the minds of some producers of vaccine, that attenuated virus could sufficiently immunize and would, further, sell better if it did not produce unpleasant effects.

Fifth—The appearance some years ago of vaccines, which had not been taken on the fourth or fifth days after inoculation of the calf, or had been taken at several successive sittings, and which necessarily contained active pus germs; or, on the other hand, a weak lymph, from animals which had not taken properly.

Sixth—Neglect on the part of the operator, even the physicians, to prepare the arm properly, prior to inoculation.

Seventh—Improper method of inoculation, the deeper tissues being often wounded during scarification, with a greater inflammatory reaction as a result.

Eighth—Neglect to protect the wound, after the vesicle had formed, against injury and the introduction from filthy garments and the finger nails of foreign matters.

As already mentioned, in most European countries the production and use of bovine lymph has taken the place of humanized lymph, due chiefly to the objections already mentioned of the possibility of the inoculation of syphilis and possibly other diseases along with the lymph.

On the American continent the use of bovine lymph is general in the United States and Canada, while in Mexico the use of humanized lymph is still the general practice. It must be confessed that the arguments for the use of humanized lymph, supplied by Dr. E. Liceaga and his Mexican confreres, in view of the experience in the United States and Canada within several years past with bovine lymph, deserve careful consideration Their arguments, briefly summed up, are:

1. The practice of vaccination, in its very nature, demands that it be performed not only by a physician, but an expert in the art, "because upon him depends the selection of the person who is to supply the vaccine." In other words, given the trained vaccinator, and he will see that seventh or eighth day lymph will be taken only from children or other persons whose health is normal and on whom there is the typical vesicle.

2. Recent experience of armies in the field, and in outbreaks of smallpox in many places, has shown but poor protection against smallpox to have been produced by inoculation with bovine lymph.

3. It is a matter of daily observation, that in Mexico re-vaccination does not "take," or only in exceptional cases, and that when it does, the explanation is due to lymph having been taken from a false vaccine pustule.

The following answer may be given to these arguments, not as refuting them, but only with the object of illustrating the problem which in practice presents itself to health officers in the United States and Canada:

First—There does not exist in practice, in any state or province, a law which supplies a body of certified public vaccinators, as in England since 1867, in Germany since 1874, in Italy since 1888, in Austria since 1900, and in France since 1902; but the remarkable fact exists, that in all those countries, while humanized lymph is allowed to be used in Scotland, in Ireland, in Austria and in Italy, yet in England, by the law of 1898 only bovine lymph is supplied to public vaccinators by the government; France has its vaccinal institute, while in Germany, where vaccination within the first or second year of life is compulsory, there are twenty-five state institutions supplying lymph at the public expense to public vaccinators appointed for every district, and re-vaccination is demanded by the 13th year, and again on entering the army. Schoolmasters, parents and others in charge of children, are liable to a fine for neglecting the vaccination of children under their charge, while severe fines are levied for vaccination performed by unauthorized persons and for negligent and improperly performed vaccination by a public vaccinator.

Second—The compulsory performance of re-vaccination in Germany, and its practice in other countries during epidemics, as well as the experience related by several experimenters in England fifty years after Jenner's discovery, but years before bovine lymph came to be generally used, indicates conclusively that at any rate in temperate climates complete immunity can only be attained by re-vaccination.

Third—The establishment of state vaccine establishments in the several most advanced European countries for the production of bovine lymph is the best evidence that where every year demands that several million infant vaccinations to be performed, the control of the supply, as regards quality and quantity, can be most conveniently and best secured by the production of bovine lymph.

Without further discussing the various points in favor of either humanized or of bovine lymph, your committee deems it worth while to inquire how far there exist exact methods which are carried out in the production of bovine lymph in the United States and Canada, or whether the situation is so unsatisfactory that some new scheme should be adopted for more adequately meeting the situation. It may be observed:

(*a*) That in all the European countries mentioned the compulsory law provides for the systematic vaccination of all children within one or two years of birth.

(*b*) That the law requires that only certified public vaccinators can perform the operation; and in England at any rate, the certificate from some public vaccination station is demanded before any medical student can obtain graduation. There the work of vaccination goes on from month to month.

(*c*) Precise directions are supplied for the performance of the operation, and especially for the seventh or eighth day inspection of the arm, and subsequent supervision of the patient.

(*d*) That heavy fines may be levied upon any vaccinator who may be shown to have been careless in performing the operation.

(*e*) That while in England gratuitous vaccination is given only to the poor, yet in Germany all the work is done at public expense, but in both cases the supervision of persons vaccinated is strictly insisted upon.

It will be apparent to those cognizant of the methods most in vogue in America, that in no one of these essential particulars, is the European practice general in any state or province. Until such regulations are systematically enforced, your committee believes that no supervision of vaccine lymph, whether produced by either federal or state governments or by manufacturers, can in any great degree obviate or mitigate results which may be shown to be directly due to the lack of regulations as already indicated for the systematic and efficient performance of the operation.

Dealing with the question of the production of vaccines, your committee, after fully considering the whole situation, hesitates to recommend the establishment of government vaccine establishments until legislation is first provided for the compulsory and systematic performance of infant vaccination. Were such in existence in the United States and Canada, it would enable the governments at Washington and Ottawa to maintain vaccine stations which would produce regularly a fairly definite amount of lymph which could be properly tested both as to purity and activity, and which, sent out to trained vaccinators, would ensure satisfactory results.

As at present there are no states enforcing general vaccination and none having the routine vaccination of children, the result is that an outbreak of smallpox occurs, agents of vaccine producers push their wares just as they do drugs, and place them in the hands of druggists and even physicians, who may know nothing of the conditions necessary for keeping the lymph; while moreover, the sudden epidemic spread of smallpox, as seen last year, often calls into sudden existence a band of medical assistants to the municipal health department, who are not trained to the work, and are likely to give their services rather at so much an hour than from the standpoint of officers responsible to the public for results.

What moreover is equally to be avoided, is the condition we have seen repeating itself time and again in recent years. Smallpox has suddenly appeared with epidemic prevalence in a number of centers at once, a general alarm is created, and the several firms producing vaccine are called upon to perform the impossible, viz., supply vaccine to the extent of ten, twenty, or even forty thousand points a day, all of which is supposed to have been produced and tested in every detail before being sent out to the public. The results we are all familiar with; but it is only fair to say, that if we are to judge from the correspondence in the medical press, the same complaints were made during the London epidemic of last year, regarding the quality of vaccine supplied in England, whether coming from private establishments or from the National Vaccine Station.

The practice and results of the work at the National Vaccine Station in London have already been referred to, but what your committee, through some of its members, has seen on this continent, in regard to the preparation of vaccine, may briefly be referred to in this report.

Apart from the City of New York local board of health, practically no lymph is produced in Canada or the United States by any but private companies. These may be such as the large wholesale drug firms who have, in several instances, at much expense, established a biological branch under expert bacteriologists for the production of vaccines and antitoxins of various sorts; or they may be firms or individuals devoted to the production of vaccine only. Apart from the question of quality, it is apparent that these drug firms, with traveling agents, have the great advantage of being everywhere at hand to supply their wares to meet any emergency, and hence are likely to, as matters exist at present, secure the larger number of such orders. Should any product prove disappointing, of course there will be a proportionately large cry for vengeance upon the firm; hence, excepting the poor results due to these vaccines in the hands of small druggists everywhere, such firms are likely to be supplying products freshly received from the vaccine farms.

Other firms will, of course, suffer similarly through local drug agents and, if a sudden call is made upon them, they would presumably, owing to limited facilities, be peculiarly tempted to send out untested lymph.

As regards the care exercised by firms in the production of lymph it may be stated that members of your committee have visited several of the vaccine farms, and while details vary somewhat, it may be said that some quite remarkable methods have been developed for the exercise of every possible precaution as regards, first, testing the health of the calves; second, their preparation prior to vaccination; third, the absolute cleanliness of their stables; fourth, the aseptic character of the operation in the inoculation room; fifth, the absence of any hay or straw or excreta in the stables; sixth, the antiseptic precautions taken in the removal of the vaccine pulp from the calves on the fifth day; seventh, the testing of the animals as to tuberculosis before putting the lymph on the market; eighth, the great care in the trituration of the pulp with glycerine in a separate chamber under aseptic precautions; ninth, the methods for storing the lymph and testing it as to freedom from extraneous organisms; tenth, the perfection of methods for the mechanical filling of tubes and the preservation of the lymph from heat during the operation; eleventh, the adoption of refrigerating methods for keeping the lymph.

What, however, everywhere has been greatly lacking, has been the final testing on persons of the quality of each batch of lymph; and owing to the absence of a routine system for supplying trained public vaccinators during routine work daily, such a lack, when a large demand for vaccine is made, must inevitably continue to some extent for the following reasons:

First—The demand of today may be ten or one hundred fold greater than one month ago, when the lymph to be used today should have been taken from the calf, tested in a calf-to-arm inoculation and in the meantime been treated with glycerine and placed away for the removal of extraneous organisms.

Second—The absence of any regular means at the several producing stations, owing to their private character and their not being a part of the municipal machinery, for a regular supply of persons for such calf-to-arm testing.

Third—The fact that the testing of the glycerinized lymph after a month, means at least another fortnight before its results can be known and that during the whole period the activity of the lymph continues to lessen somewhat, makes it a necessity that the lymph be sent out at the earliest moment after the extraneous organisms have been removed.

Fourth—Should there be any practical danger that such a germ as of tetanus can be in the calf then the delay in sending it out, until the experiment in a person shall have been made, would be yet more demanded. Fortunately, however, in the opinion of your committee, there has not been a single fact which can fairly connect any of the unfortunate cases of post-vaccinal tetanus with the presence of the germ in the vaccinifer, and therefore such delay does not become in practice a necessity; while the possible contamination of the lymph during the preparation of pulp with glycerine would seem to be very slight, compared with the danger to which people are exposed daily from the inoculation of wounds with dirt or with dust from the air.

Recent experiments at the National Vaccine Station, London, moreover, have shown that by the treatment of the vaccine pulp with chloroform and boracic acid, the extraneous organisms may be removed in from twenty-four hours to one week; and hence there is every reason to hope that lymph free from extraneous organisms will soon reach the market several weeks sooner than has hitherto been possible for tested glycerinized lymph.

So far as your committee is able to judge, no practical scheme can be adopted by a state or provinical board of health, under existing legislation, for the supervision of the lymph to be used within its jurisdiction, unless it be that state boards would agree to recommend for use by their local boards of health only the lymph of such firms as will agree to pay for the presence in their establishments of a trained officer, who could certify that all lymph sent to such state or province, had been tested and found to meet all the requirements, which science today demands for the preparation of a lymph of the highest degree of excellence.

Whether such a scheme would in practice be possible, is not for your committee to express an opinion upon, since the element of commercial competition would have to be considered; but the experience of many executive officers, both state and municipal, has been such as to make the adoption of such supervision if practicable eminently desirable.

(Signed.) P. H. Bryce,
Wm. M. Welch,
Eduardo Liceaga.

RESULTS OF REVACCINATION IN MEXICO.

By Dr. JOSE RAMIREZ, Mexico, Mex.

The Mexican delegation has on numerous occasions reported to this association on the brilliant results that have been obtained by the employment of human vaccine. In the paper that I had the honor to read at the meeting at Buffalo in the year 1896, I presented a diagram in which I attempted to show that by the human vaccine which we employed in the City of Mexico we had been able to avoid the appearance of smallpox in all the individuals who had received that benefit. Since then, Dr. Eduardo Liceaga has presented another paper with fresh data and new ideas, in which he further showed the advantages that had been obtained in Mexico through the employment of human vaccine.

By way of further proof of what is stated in the previous papers, I now come to report to this association the results of two experiments that have been carried out under the directions of the Supreme Board of Health, and which prove in the clearest manner that human vaccine gives permanent immunity, and therefore renders unnecessary any periodical vaccinations. These experiments have consisted in the revaccination of all the children resident in a public philanthropic institution that is under the management of the government and that is known in our country as the Poor Asylum, and in the revaccination of 661 individuals in the city jail. In the first experiment, we revaccinated 215 males and 431 females, making a total of 646 persons of the following ages: From 4 to 10 years, 315; from 10 to 20 years, 302; from 20 to 30 years, 25, and over 30 years of age, 4.

The results obtained were as follows:

	Males.	Females.	Totals.
Positive in	7	4	11
Negative in	151	226	377
False vaccination in	57	195	252
Doubtful in	0	6	6
Totals	215	431	646

Among the revaccinated persons in the asylum, there was one individual who bore traces of a confluent smallpox, and on being revaccinated, a positive result was obtained.

The second experiment was made in the city jail by the revaccination of 661 persons, of whom 53 had been vaccinated from 10 to 15 years previously; 62 from 15 to 20, and 546 from 20 to 50 years previously. The results were as follows:

	After 10 to 15 Years.	After 15 to 20 Years.	After 20 to 50 Years.	Total.
Positive in	1	1	12	14
Negative in	37	43	388	468
False vaccination in	15	18	146	179
Totals	53	62	546	661

I would call your attention to the important fact that of the 14 who gave positive results, 12 were individuals who had been vaccinated from 20 to 50 years previously, and to the still more important fact that of these 12 individuals 2 presented traces of confluent smallpox.

Besides this, 61 individuals were revaccinated in the central station, of whom one was an American, one Argentine, 14 Spaniards, 6 Frenchmen, one Englishman, 4 Italians and one Peruvian, all of whom had been previously vaccinated with animal lymph.

Of the positive results obtained in the asylum, two individuals were five years of age, one six years, four nine years, one eleven years, one twelve years, one thirteen years and one seventeen years old. Of the twenty-eight foreigners who were revaccinated, none gave positive results; but it is to be noted that all of them had been revaccinated several times and recently.

The above figures, which are worthy of consideration, will clearly show that human vaccine has sufficient efficacy to secure immunity for life to the immense majority of the individuals who submit themselves to its action.

THE ASEPSIS OF VACCINE.

By Dr. JESUS GONZALEZ UREÑA, Mexico, Mex.

> The operation of the vaccination will always be preceded by the asepsis of the arms, of the vaccinated parties, of the surface of the pustules that supply the virus, and the hands of the operator, as well as the sterilization of the instrument which is employed.—Board of Health of Mexico.

Vaccination, like all well practiced surgical operations, is now free from the complications that so often accompanied it in the pre-antiseptic period, and this result is all the more beneficial because this wonderful preservative is still with difficulty accepted by the uncultured classes of society, who formerly saw in erysipelas, in lymphangitis, in phlegmons, etc., that often appeared during the evolution of the pustules, tremendous arguments against one of the most useful discoveries that medical science has ever made.

The suppression of these serious accidents has, in the course of time, eliminated from the public mind the prejudices that were so rooted, and that rendered necessary in many parts of the world, including Mexico, the enactment of laws declaring vaccination obligatory, so as to establish it to the extent that was required. If the prophylactic virtues of the Jenner vaccine lymph are not understood by narrow-minded people, everybody can understand the innocuous character of its application, and this has gone a long way to facilitate its propagation amongst those persons who formerly opposed it.

The Supreme Board of Health of Mexico, which has adopted every possible means within its power to popularize vaccination, promptly took advantage of the new discoveries made by Pasteur, and in Subclause V of Art. 8 of the regulations for the administration of vaccine in the federal district and in the territories of Tepic and Lower California, it has laid down the precept that forms the heading of this paper.

During the three years in which I have personally observed the results of these regulations, in the capacity of sanitary inspector of the district of Guadalupe Hidalgo, I have come to the following conclusions, which I desire to bring before this learned Association.

There is no branch of surgery in which we are more justified in the abandonment of the means that properly are called antiseptic for the more simple precaution of asepsis, because, considering the slightness of the operation in itself, whose technique is reduced in Mexico to a simple subcutaneous inoculation that is almost painless and bloodless, and executed with a narrow-pointed lancet, we can easily understand how small an opportunity is presented to the germs for the production of an infection, especially when the surface of the skin has been mechanically removed by the washings that should be made with sterilized water and soap, and when they are not found on the hands of the operator, with which similar precautions have been taken, nor on the instruments employed, as these are sterilized by ebullition.

But if the antiseptics did not present any other objections already known to surgeons, we would find a sufficient argument for not employing them in vaccination, in the attenuating and destructive influence which they exercise on the lymph. In a series of 11,145 persons whom I have vaccinated during my practice of three years in the above district, I proposed to use one of the mildest antiseptics, which was boric acid in an aqueous solution of three per cent., which we every day placed in contact with the most delicate mucous membranes, without ever finding the slightest irritation in consequence, and I very soon found myself bound to suppress its use because I observed that in many cases the pustules did not acquire a good development, whilst in others, when they did appear, they were small and looked to be atrophied.

Since then I have strictly complied with the legal provisions that form the heading of my paper, and the publication of which was subsequent to the experiments above related. I confine myself entirely to the asepsis, and with the result that in the 705 vaccinated persons whom I have been able to follow, not only did I observe no complication of any kind during the vaccinal evolution, but I also observed that the indications of local inflammation which are characteristic, the areola, tumefaction of the base of the pustules, elevation *in situ* of the temperature and pain, were greatly attenuated, without in any way diminishing the size and good quality of the eruptive elements, or their immunizing properties. The same thing happens with the symptoms of general reaction, uneasiness, fever, etc., which supervened from the sixth to the eighth day after vaccination, and that when they appear cannot pass unnoticed, even by the most careless persons, but so mild is the aseptic vaccine that several children who were not carefully looked after by their parents were presented to me after the eighth day after inoculation, under the supposition that the operation had not been successful, but on making an examination of the arms, I found to my surprise splendid vaccine pustules, which showed that they had gone through a silent revolution and that the asepsis had deprived them of their local and general phenomena of reaction.

I therefore close my paper with the following proposition:

First—In the practice of vaccination, only asepsis is to be employed, because experience shows that it is alone sufficient to render the operation innocuous, and also because the employment of antiseptics, even the mildest, debilitates the Jenner lymph.

Second—Asepsis eliminates the complications that were formerly observed in vaccination, and without prejudice to its beneficial effects attenuates the general and local reactionary symptoms which are characteristic.

REPORT OF COMMITTEE ON SANITARY AID SOCIETIES.

By Dr. GEORGE W. GOLER, Chairman, Rochester, N. Y.

The Chairman of the Committee on Sanitary Aid Associations, Dr. Goler, and the other Rochester member, Henry Lomb, have not been able during the last year to formulate plans for the organization and practical working of such societies and to propose to the other members of the committee such a plan. The cause of this has practically been that the chairman, who is the health officer of Rochester, has been for some time, especially during the appearance of smallpox in the city, overburdened with his professional and official duties and work, and for the last two months has been absent in Europe. Another reason has been that it was considered advisable to see how successful some of the work of the Rochester Public Health Association would prove before making propositions for other societies. The two Rochester members of the Committee on Sanitary Aid Societies desire to submit the report of some of the work done by the Rochester Public Health Association, and the paper read before this association giving suggestions for its operation, as their report of your committee.

The Committee on Sanitary Aid Societies begs leave to submit the following as a report of some experimental work done in the city of Rochester, N. Y.

The work done at Rochester, N. Y., has been a direct outcome of a local organization known as the Rochester Public Health Association; through this organization, made up of representatives from each ward in the city, a series of illustrated lectures was arranged during the past year. These lectures were on sanitary subjects, chiefly relating to housing problems, sanitary disposal of waste products in cities, sanitary cooking, and the prevention of contagious and infectious diseases. The lectures were given at many different school houses before the mothers, teachers and parents clubs connected with the different public school organizations, and before a number of different church societies in the church lecture rooms. The lectures were given by physicians and by one of the faculty of the local university. Applications were made for the lectures to the secretary of the local society, and the lectures were given without charge to the societies applying for them. Applications for these lectures from churches and schools were very numerous, so much so that the staff of lecturers was not sufficiently large to do all of the work. The lectures were well attended.

Stormy nights and the season before the holidays did not appear to prevent a large audience from attending the lectures. The success of the local society's efforts during the first year of its lecture work led it to believe that another year the lecture work can be considerably extended. Through such means the local society hopes to educate public opinion so that it may demand better sanitary surroundings for all dwellers in cities, to the end that life may be made freer from bodily illness and all human happiness promoted.

During the past year 48 lectures have been given by Dr. Goler, Dr. Leary, Dr. Weigel, Prof. Dodge, of Rochester University, and Mrs. Caldwell, teacher in the Mechanics Institute.

Sixteen lectures on "Some Rochester Homes," illustrative of unhygienic conditions.

Three lectures on "Foods and Food Supplies."

One lecture on "Smallpox in Rochester."

Six lectures on "Foods for Children, Value and Preparation."

One lecture on "Bacteria in Health and Disease."

Five lectures on "Physical Development of School Children."

Seventeen lectures on "Contagious Fevers of Childhood; Dangers and Prevention."

The following list of lectures is ready for the season of 1902-3, and others are in preparation:

The Teeth in Health and Disease.
Food for Children.
Smallpox and Vaccination.
How Some Rochesterians Live.
Bacteria and Disease.
Diet and Health.
Skin in Health and Disease.
Children's Diseases—Cause and Prevention.

Lantern lectures are preferably given in the evening.

The Association will also in a short time offer a free inspection of the teeth, the defects to be noted on cards, and the person referred to their dentist. This is intended as a beginning of thorough instruction in dental hygiene.

SUGGESTIONS FOR WORK OF SANITARY AID SOCIETIES.

One of the first objects—in fact, the basis of operation of the new organization—should be to assist in creating or promoting a public opinion which will consider the *preservation of health* and the *prevention of disease*, two of the most important and essential elements for comfort and happiness in life, and which will bring to individuals, families, and the public, the conviction that it is the duty of everyone to assist in such work and measures as may tend to preserve and *promote* health and *prevent* disease.

The following seem to be practical means for attaining this end:

First—A healthy coöperation with local and other health authorities and organizations.

Although this organization will be an independent one, and reserve to itself such action as the judgment of its members may decide to be proper, yet it should be guided in a great measure by the advice of the health authorities and should assist them in their work, in the distribution of publications, and in the enactment and enforcement of such laws and measures as may be conducive to the realization of the objects of this organization. The organization should also exert its influence partly through the different ward organizations, in behalf of the selection of members of the health boards, of city physicians and employes of the health authorities, of persons who by their character, ability and knowledge are well fitted for their respective positions. The organization should also assist in obtaining from the respective authorities, for the health authorities, the adequate means for carrying on necessary and efficient work.

Second—The distribution of publications of our local board of health, and those of other cities, states and sanitary organizations .

After the preparatory steps for the formation of this organization were taken, a letter was sent to the American Public Health Association, to different state and city boards of health, and other health organizations, giving an outline of the objects and work of our proposed organization, and asking for copies of their publications. The responses were very hearty in approval of our movement, and we have now a large number of pamphlets, of which very large numbers have been distributed among the public. Some sent their regular annual reports, with proceedings of meetings. These publications are the work of able men and women, sincerely devoted to the cause of the human family, and are the products of years of earnest, hard study and long experience. Most of them relate to the *prevention of disease* by a reasonable knowledge and observance of the laws of health, hygiene and sanitation. As it is very natural and proper that we should take advantage of the experience of others, it will undoubtedly be decided by our organization to reprint such of these publications as may appeal to the publication committee, when appointed, as suitable for the purpose. Through the different ward organizations the distribution can be done very effectively, and while some will go only to those specially interested in the subject, most can be sent to every house in the city.

Third—Providing for suitable properly-arranged rooms, for meetings, and to be supplied with the publications mentioned, with periodicals and books, also with models, charts and specimens, showing proper and improper drainage, plumbing, ventilation, apparatus used for prevention of transmission of disease to others, and anything else relating to the objects and aims of this organization. This would mean a home with well-provided reading room and a nucleus of a comparatively small, but practical museum of hygiene. Arrangements have already been made to secure such rooms.

Fourth—Popular, practical lectures in different parts of the city, if possible in every ward, relating to the objects of the Association.

Part of them should be specially arranged for the two or three upper grades of the schools, part for the public. With these lectures should be connected exhibitions of lantern views, models and specimens of proper and improper work in the line of the subject of the lecture. Tickets to these lectures should be of only nominal cost, or entirely free.

Fifth—Inspection of houses and other buildings.

To some extent this is done now by our board of health, but principally only in cases where complaint is made, generally by neighbors, and in some cases of contagious diseases. This organization should, if possible, assist the board, if necessary, in these inspections, but it should also make arrangements to have, independently, inspections of the sanitary conditions made by competent and conscientious experts. If a regular, practical system of such inspections were established, the expenses of same would not be very great, and the owners or occupants of houses who are able to pay, should be charged with about the cost of the work, while in some cases, where conditions seemed very dangerous to health, and where no means for paying the expenses exist, the organization should bear them. The inspection would principally relate to drainage, plumbing and ventilation, but able and faithful inspectors would undoubtedly find, in certain cases, conditions relating to other matters, where beneficial changes, sometimes at a very small cost, could be made, and they could give tactful, practical advice which, at least in some cases, would be followed. That there exist strong reasons in favor of house inspection, even in costly houses, will be attested by physicians and other experts, after an inspection following cases of sickness or death directly traceable to unsanitary conditions of houses or surroundings.

Sixth—To make special efforts to prevent the spread of contagious diseases, particularly consumption.

In different parts of our country, and also in foreign countries, strong efforts have been and are being made, partly to prevent the spread of consumption, partly to cure it, by establishing special hospitals for those afflicted with this dread disease, which is the cause of the death of so many. This organization will assist, by its educational work of distributing circulars relating to this and by arranging for removals of some of the afflicted from their homes to hospitals, if possible to special hospitals, thus giving the patient a better chance to recover, and at the same time secure the other members of the family from the danger of contagion. It is believed that some of the most beneficial work of the organization will be in this direction.

Seventh—The coöperation with other, especially charitable organizations, educational authorities, teachers, school children and employers.

While the information from the charitable associations, regarding cases of sickness and the conditions of houses, would be very important, this organization could also assist them by furnishing in extreme cases of sickness, nurses or other practical help. A very important factor of success would be the coöperation of the educational authorities, teachers and school children with this organization. Much could be done by them in distributing documents and information, and in creating an interest in their own and their friends' homes in the work of the association. On the other hand, this organization, by inspection of school houses, lectures, and other means, may be able to do considerable for the cause of education. If a joint committee of the members of the board of education, teachers and members of this association could be appointed to formulate a plan for coöperation, undoubtedly good results could be obtained. The coöperation of employers would be very desirable, especially to establish a system which will prevent the transmission from an employe afflicted with consumption of the disease to other employes. Although this is a very delicate matter, there seems no doubt that employers will coöperate in the protection of those under their charge.

REPORT OF COMMITTEE ON THE TEACHING OF HYGIENE AND GRANTING A DIPLOMA OF DOCTOR OF PUBLIC HEALTH.

By Prof. W. T. SEDGWICK, Chairman, Boston, Mass.

The principal hindrance to the development of proper instruction in hygiene is the fact that the organization of American boards of health has not hitherto been and is not generally now such as to furnish an adequate livelihood to physicians or others specializing in the direction of public health work. The same fact explains why there has been no demand for the degree of Doctor of Public Health. The possession of such a degree, at least until very recently, would have had simply an academic significance. It now seems possible that delay has not been without its compensation, for with the development of sanitary engineering and bacteriology numbers of young men are now being trained as sanitary engineers and as bacteriologists who may very likely become more useful in the future in the organization and work of boards of health than "Doctors of Public Health" would be likely to be. The Doctor of Public Health must be, of course, in the first place a physician, but for the work of a modern board of health, the training of a physician would be only the beginning of his needful knowledge. If he is to cover the whole field of sanitary work he must be also a fair engineer, and a good sanitary as well as medical bacteriologist. Now if such men existed fully prepared for their work we should have to confess that very few places would be open to them yielding salaries at all adequate to their attainments.

As for myself, while I have felt until recently that the degree of Doctor of Public Health would be eminently desirable and proper, more mature reflection leads me to hesitate somewhat in pronouncing a final opinion on the subject. I do not feel at all sure that the organization of sanitary work in the United States is not likely to proceed with advantage along new and different lines and that it may come to pass that thorough-going medical men will serve as medical inspectors, while bacteriologists and sanitary engineers will be called in to make many of the necessary investigations and to advise as to remedial measures.

The questions at stake are not only very interesting but very important. There can be no doubt of course that proper instruction in hygiene is one of the pressing needs of the time. The field was never whiter for the harvest, but the laborers are unfortunately few. The reason is, no doubt, the fact that positions have been few or altogether wanting hitherto for those who might have been willing to give their lives to this work. Some change has been wrought recently and in a very hopeful direction by the establishment of municipal laboratories, mainly bacteriological, largely for purposes of diagnosis and as practical aids to boards of health. Progress in this direction has been so rapid that it seems to me more than possible that the natural development of the subject in the United States will proceed from the laboratory as a starting point rather than along the lines which have long existed in England.

I would suggest to the committee that inquiries be made during the coming year: First, as to the instruction now obtainable in America in hygiene both public and personal; second, as to the positions available for experts in hygiene, sanitary engineers, bacteriologists, doctors of public health if such should be created, etc.; third, as to the extent to which medical inspectors are already employed by state and local boards of health; fourth, as to the growth of municipal and state laboratories of hygiene and the ways in which these may be made to supplement the work of medical inspectors.

VITAL STATISTICS: A PLEA FOR ACTUARIAL ADMINISTRATION AND CONTROL OF THE GREAT RESOURCES OF PREVENTIVE MEDICINE.

By JOHN S. FULTON, M. D., of Baltimore, Md.

In the arts and sciences it is commonly observed that men's failures are oftenest traceable, not to want of knowledge or experience, or of ingenuity or logic, but to imperfect mastery of five-finger exercises. A sort of drunkenness which we call "enthusiasm" inflates us with a scorn of 'prentice tasks and with the conceit that we may go ahead *per saltum.* Consumed with haste to be master workmen, we acquire a wealth of implements and abandon the education of our thumbs. When the too obedient tools bring us to derision, we are driven back to the finger drill that will make one a craftsman probably, and at length a master, perhaps. This propensity to neglect the mechanical adjustment of one's self to one's materials, and to be supplied with every sort of unthinking machinery, particularly afflicts the modern practice of preventive medicine.

Preventive medicine has, within a generation, acquired territory from every adjacent science. Crafstmen possessing special and peculiar training have entered the service of hygiene, bringing the queer tools and unfamiliar methods of small provinces in science, speaking the provincial dialects, not understanding each other, not well enough understood to be assigned definite metes and bounds within a new environment. The offerings are valuable, but their profusion is bewildering. Another generation may pass before the serviceable energies of the chemists, physiologists, bacteriologists, geologists, meteorologists, engineers, economists, architects, sociologists, entomologists, and actuaries are safely geared to the machinery of public hygiene. Among these new forces some are strong enough to run the whole machine, and that possibly not to smash. The commissioned officers of hygiene are so much impressed with the performances of some of these specialists as to have forgotten, apparently, that the theory and parctice of hygiene rest upon an older science, which in some form or other is fundamental to every other science, and that the several underpinnings must be compacted into a solid foundation upon which the complex and ponderous machine can be made to run steadily.

Public hygiene is built upon, is controlled and directed by, and is everlastingly in debt to vital statistics. The might and the right to direct the future of preventive medicine, to make and to terminate contracts, to approve and reject risks, to test materials and methods, to invest means and to distribute profits, these things belong inalienably to vital statistics. Every wheel that turns in the service of public health must be belted to this shaft, otherwise preventive medicine must remain invertebrate and unable to realize the profits available from the magnificent offering of collateral sciences. If the unborn historian of hygiene in the twentieth century shall find one anomaly more curious than any other, it will be that the twentieth century, opening with prodigious resources, immediately available, ran a third or half its course before these resources became so standardized that each unit of power might be accounted for in a definite scheme of vital statistics.

It is for the immediate articulation of the statistics of public hygiene that this paper pleads, and against the misuse of such statistics as we have that protest is made. The situation is not absolutely bad. Indeed, we might congratulate ourselves upon progress, if the statistic spine of preventive medicine were not so far outgrown by other parts of the organism.

THE FEDERAL CENSUS.

The foundation of vital statistics rests upon the federal census, which we seem to have agreed shall be taken once in ten years. A decennial enumeration of the population, properly made, skilfully analyzed, and promptly published, will suffice for the primary needs of sanitarians. The establishment of the United States Census Bureau on a permanent footing will enable the medical profession to impress with some effect the importance of certain reforms which have hitherto been asked in vain, and there is ground for hope that the particular information needed by hygienists will in future be better collected and better presented.

However satisfactory the census reports may have been to those who had most influence in formulating the details of the work, never a census has furnished the data most desired by sanitarians. Medical statistics were not attempted until 1850, and not once since that time has a crude death rate for the country been determined. The census office determines the population of the country as nearly as possible on a given day, usually in June (*i. e.*, the number of survivors of the census year); but does not determine the mean population for the year ending on the assumed date of the census, and this mean population is the true number of those who furnished the mortality up to the last day of June in that year. This gives too large a divisor. The dividend, on the other hand, has always been too small, representing in large part the results of the enumerators' efforts to collect the data concerning deaths for a year by inquiry among the living at the end of the year. We have, therefore, no statistics comparable with the statistics of other countries. But, what is worse, we are unable to make comparisons between separate areas in our own country at the same period, or even to compare the data of different periods for the same area.

The census officials have not been blind to the futility of the enumeration method of collecting death returns in non-registration areas. On the contrary, in the reports of the tenth and eleventh census the results are repeatedly criticised by Dr. Billings, as indicating not the mortality rates, but the more or less defectiveness of enumerators' returns.

The blunder was nevertheless repeated in the twelfth census, and that against the protest of a great majority of the registration officers of the country. A committee of the American Public Health Association strongly urged upon Congress an amendment to the last census bill giving the director of the census discretionary power to make records of all deaths occurring within representative areas of non-registration states for a period of one year, and to apply the derived ratios of these data to the entire population of the state so handled. In this way complete and satisfactory records of all deaths, within a measured period and in a space of representative constitution, might have been made by competent observers at the time and place of their occurrence. With fresh and exact data, all the rates computed for the registered portion of a state would have been trustworthy, and when applied to the whole state would have given approximately true figures. This amendment, if utilized as the language of the amendment permitted, merely in a tentative way, being operated in selected portions of but one or two non-registration states, would have served as a check upon the work of the enumerators, and, coming into comparison with the older practice, would have allowed a reliable judgment of its own merits.

But when the amendment was under consideration no participant in the earlier errors was at hand to plead for better methods. The census office was for the twelfth time to be organized *de novo*, and although the humble confessions of previous failures were quoted in the memorial which accompanied the amendment, the National Legislature, taking no counsel of its own commercial wisdom, rejected the advice of the sanitary bookkeepers. The data of the twelfth census are therefore no more applicable in the non-registration states to the work of public hygiene than those of the eleventh census.

For the "registration area" of the twelfth census the statistics may be accepted as fairly approximate but the population is excessively urban, and includes so many new cities that the group as a whole cannot be taken as representative of the population of the country. The census office divides the registration states into two groups, those whose returns of deaths, being collected under stringent laws, may be accepted without further inquiry, and those whose returns, when compared with the enumerators' returns, show more or less incompleteness. Of this latter class those whose returns, when compared with the enumerators' return, show a difference not exceeding 10 per cent., are taken into the statistic account, and with the first class constitute the "registration area," which the census office utilizes for the purposes of comparing the results of the twelfth with those of the eleventh census.

The eleventh census determined its "registration area" upon a looser standard. Its list of states included Massachusetts, Connecticut, New Hampshire, Vermont, Rhode Island, New York, New Jersey, Delaware and District of Columbia, and to these were added 83 cities outside those states, giving in all a registration population of 19,650,440. Delaware is very properly excluded from the "registration area" of the twelfth census, and Maine and Michigan are added to the list, together with 153 cities of 8,000 or more population in other states, giving in all a registered population of 28,807,269.

In but four of these states registration was found to be complete enough in 1900 to allow the enumerators' schedules to be withdrawn and local returns to be substituted therefor. These four states were Maassachusetts, Connecticut, Rhode Island and New Hampshire.

In New York, New Jersey, Maine, Michigan and the District of Columbia it was found that 20 per cent. of the enumerators' returns were of deaths not registered in the locality. Admitting, for the moment only, that the local registration could be safely amplified by the enumerators' returns, it must be clear that the enumerators' returns are subject to two undetermined discounts, one for errors of total omission, and the other for errors as to the time of death. Enumeration experience in 1880 was carefully tested, and the deficiency on account of records wholly missed amounted to 30 per cent. The census office in 1890 hinted at an even greater deficiency. Of the discount for error in time nothing is known except that it is greatest in the earlier months. The statements of those interrogated concerning the occurrences of exactly a year past are at best inaccurate, and when the inquiry extends from June to June, so including parts of two calendar years, considerable error must result. Perhaps the registration in these ten states was better than the census office was led to believe. Certainly the registration of the four New England States was reliable, and if these returns were applied to a correct population for the period of their occurrence the rates would be useful.

But in 1890 there were not four New England States whose returns were complete. Registration in Connecticut, New Hampshire and Rhode Island was in 1890 no better than in New York and New Jersey in 1900. The populations of these several states are, however, fairly homogeneous, do constitute the best registration area which the country affords, and deserve a separate statistic treatment which the federal census does not give.

By including in the registration population 183 "registration" cities in non-registration states the statistic results are distinctly vitiated. Among these "registration" cities are many whose populations are of abnormal age, race, and sex constitutions, and some whose records are subject to a sort of vicious manipulation which will presently be considered.

The death returns made by the health departments of the cities in non-registration states are said to be "corrected" by the enumerators' returns. This simply means that the local official returns are amplified by the number of those enumerators' records which are not found in the local official returns. Before the twelfth census no offer of copies of these excess returns was made to local officials, although such copies would have been of service to the local officers. If they were found correct, the sources of failure in the local registration might have been discovered, while, if found incorrect, the errors in enumerators' returns would have been exposed. The excess enumerators' records of the twelfth census were offered to the local registrars, but upon condition that no prosecutions at law should be based upon them, and that they should be paid for at the rate of two cents each. It would seem clear that a verification of these unregistered deaths was of no less interest to the census office than to the local registrars, and a free tender of the list would have been fairly compensated by a report upon the results of investigation by the local registrars. The local registrars to whom the census offered these bargain-counter remnants were of two classes: those who preferred that the matter should remain in doubt, and therefore did not want the lists, and those who did not want the lists because they were quite sure that, except by the hazard of grave crime, no death within their district could escape registration.

Eventually the census office concluded to offer the lists free of charge, and several hundred excess enumerators' returns were sent to my city in a non-registration state. Their investigation is now in progress. Up to the present time it can only be said that the chief, if not exclusive, source of error is, as might have been expected, in the duplication of records. The enumerators' records have been easily identified as more or less erroneous returns of registered deaths, whose true data, copied from the official records made at the time and place of the events, had been sent to the census office. In this way one death entered twice into the census enumeration, figuring once perhaps in each of two disease columns, or of two age columns, or month coulmns, or once in a residence and once in a hospital.

There are "registration" cities where certain deaths are thrown out of the list because there are plausible reasons why they should not be counted (*e. g.*, deaths of non-residents), and certain other deaths are excluded from the city registry for reasons just as plausible (*e. g.*, extraurban deaths of citizens); where the control of the death rate lies partly in the count, but chiefly in the discount. The returns of these cities really needed the enumerators' correction.

In view of all the sources of error and of the non-representative character of the several areas whose stastistics are combined and compared, it savors somewhat of hardihood to offer the derived ratios as indices of profit in public health. We are told that in 1900, 341 "registration" cities of 8,000 and over gave a combined death rate of 18.6 per 1,000, while in 1890, 271 "registration" cities gave a combined death rate of 20 per 1,000, showing a gain of 2.4 per 1,000, or 11 per cent. in ten years. The average age at death in 1890, we are informed, was 31.1 years, while in 1900 the deaths in the total population of 28,807,269 included in the ten "registration" states and 153 "registration" cities in non-registration states, occurred at the average age of 35.2 years. The alleged 12 per cent. increase in the duration of human life is offered for our admiration. To mistake the average age at death as indicating the mean duration of life is a common error of the uninitiated, but no self-respecting expert can afford to wink at such a blunder, for it rests upon the assumption that populations are, in a qualitative sense, fixed or stable, while it is an elementary principle of vital statistics that populations are unstable, and that all determinable variations must be taken into account. The qualitative variations of population, even in a country as old as Great Britain, are so wide that Chadwick was able to make the generalization that in healthy districts the mean age of the living is, as a rule, less than the average age at death, while in unhealthy districts the reverse is the case. As an extreme instance of the variations found in practical experience, one may cite the observation of Rumsey, who found in London in the fifties a death rate of 24 per 1,000, an average age at death of 29 years, an average age of living 26 years, and an average expectation of life reaching to 37 years, all in the same population at the same time. What, then, shall be said of the ratios derived from the incoherent heterogeneous data of the twelfth census? The crudest two-factor rates of an unschooled registrar are not so crude.

All this is not, however, to be imputed to the census office for sin. The failures were for the most part inevitable in the nature of the case, and they are exhibited here not so much for reproof as for exhortation. The difficulties of enumeration by the federal census office of deaths in nonregistration areas were and ever will be insuperable. Enumerators' data should be tabulated and displayed without any correction by local returns, and so brought into amazing contrast with that one nineteenth part of the Union whose death records are collected by local officials day by day, year in, year out.

The census office now maintains a continuous organization for purposes more numerous and complex than were formerly contemplated. The first census was a political necessity in order to apportion the congressional representation of the several states, and while the value of vital statistics has been somewhat neglected in the development of the incidental advantages of a federal census, there is now good reason to believe that this subject will receive adequate treatment. The vital conditions of large areas will perhaps for some decades have to be investigated by the census office through its own agents and without the aid of local official registration. The discredited enumeration method was confessedly as disappointing to the statisticians of the twelfth census as it was to those of the tenth and eleventh, so that we may expect some plan to be devised, and perhaps perfected in the intercensal period, whereby the vital conditions of nonregistration areas may be approximately determined. It would seem that the registration data of some part or the whole of the District of Columbia could be utilized for comparison with the data obtained by census officers in some comparable areas in one of the contiguous nonregistration states.

For quite reliable results we must wait upon the will of the people to record for themselves the vital statistics of their several sovereign states, and the census office has, as we shall see presently, joined heartily in a movement to extend the practice of local registration.

STATE REGISTRATION OF DEATHS.

To one who realizes how great a structure of practical hygiene is delayed for want of a suitable foundation in vital statistics, the growth of the practice of registration seems dismal. But the registration idea has in fact spread rather rapidly. In 1880 real registration under operative laws was practiced in Massachusetts, New Jersey, the District of Columbia, and in 19 cities in nonregistration states. In 1890 the practice of registration had extended to five more states, and the number of registration cities in nonregistration states had risen to 83. In 1900 there was some sort of registration of deaths in 37 states, and in 16 of them the registration was as good as it was in the nine states accredited by the eleventh census as registration states.

But the registration idea has not produced similar or comparable results in all of the states where it has taken root. Indeed the spread of registration has brought into painful prominence the greatest diversity both of methods and results. The hopeless process of an annual census of deaths is yet in use in five states, and the results range from tolerably poor in Vermont to wholly worthless in Kansas. The great states of Pennsylvania and Ohio are in this class. In 28 states physicians, or other persons acquainted with the facts, are "required" to make returns of all deaths within periods of from 24 hours to a year after their occurrence, but these "requirements" are practically inoperative in about 20 of these states. Fifteen states have discovered that the burial permit will insure registration at the time and place of death, but of these 15 we must count off eight as having failed to insure the burial permit. Of the remaining seven, Indiana insures the application for a permit to bury by authorizing the state board of health to disinter and interrogate the corpse. This resurrection feature is entirely novel in the practice of registration, but it is said to have made Indiana a registration state, without having caused an uprising of the silent majority. The states whose burial permit laws are effective are no more than seven.

Diversity of practice appears also in placing the official responsibilities. Local (county or township) registration officers may be county clerks health officers, auditors, assessors, recorders of deeds, or orphans' court officials. State registration is centralized sometimes in the state board of health, again in the department of state. No sort of uniformity appears in the publication or tabulation of vital statistics, and the blanks and other printed forms employed are of almost infinite variety as to size, arrangement and contents.

The idea of permanent preservation of the records has not been strongly impressed upon the official mind except in one or two states where ancient records are now either wholly lost or under process of restoration at great expense.

A strong movement has been made against all this neglect and incongruity, and in respect at least of a definite scheme of classification of causes of deaths, notable advance has been made. A number of experienced registration officials with the aid of expert statisticians and hygienists undertook in 1897 to bring about a general agreement among American registration offices to employ the Bertillon classification of diseases and causes of death for statistical purposes. The claim was never made that this classification meets exactly the requirements of modern pathology or that its intrinsic merits for any other than statistical uses are surpassing. The Bertillon scheme had, however, at the time a good prospect of receiving international recognition, having been planned with this in view, and its general adoption in this coutry offered the best prospect of securing a practical basis of comparable statistics. The subject was effectively presented at successive meetings of the Conference of State and Provincial Boards of Health, the American Public Health Association, and the American Medical Association, and its adoption by registration areas, new and old, followed. This movement received a further impetus at the International Medical Congress at Paris in 1900, when the Bertillon classification was revised by a committee including members from all parts of the world, and was approved by the congress as a practical international classification for statistical purposes. The United States Marine-Hospital Service published and distributed a good translation of the scheme. The director of the twelfth census adopted the scheme, and with the establishment of a permanent census office the Bertillon classification became the authorized plan of account in American vital statistics.

As contributing towards the extension of the practice of registration to new areas, and toward needed reform in states where registration is more or less defective, the census office has printed and distributed a circular on the subject ("Vital Statistics, No. 71"). The opening words of this circular are significant: "The Census Office is primarily interested in the registration of deaths because the data required for the mortality statistics can only be obtained in complete and reliable shape where deaths are recorded *immediately after their oecurrence*, under compulsory registration laws. It is impossible to secure a complete record of deaths *in any other manner*."

The important part of the circular is a paper prepared by the committee on demography and statistics of the American Public Health Association, and the title of this paper is "The Essential Requirements of a Law for the Registration of Deaths and the Collection of Mortality Statistics." This brief and well considered document states in logical order the several features which are absolutely essential to a working law, eight in number, as follows:

1. Deaths must be registered immediately after their occurrence.
2. Certificates of death should be required.
3. Burial or removal permits are essential to the enforcement of the law.
4. Efficient local registrars are necessary.

5. The responsibility for reporting deaths to the local registrar must be fixed.

6. The central registration office should have full control of the machinery, and its rules should have the effect of law.

7. The transmission and preservation of the records should be provided for.

8. Penalties should be provided.

Barely two pages are devoted to the statement and explanation of these eight points. Where experience has approved more than one method of accomplishing an essential result the alternative procedures are clearly described. This brief document is, as it professes to be, an authoritative guide, and any competent attorney should be able with its aid to formulate an effective law for the registration of deaths in a state where the subject is wholly new. The appendix to this interesting circular shows again good results of consultation and coöperation between federal and local statisticians. It describes a standard certificate of death, which provides for all the data essential to the work of the census office and to the private and local uses of such records.

Among the other evidences that the census office has now a definite grasp of this important subject, one should mention the "Manual of International Classification of Causes of Death," published in November, 1902. No such handbook has heretofore been available to American registrars, and it will at once take rank as indispensable. Its contents deserve extended notice, but we can only allude here to its double index to the classification of causes of death as returned in the census of 1900. Very many of the perplexities encountered by registrars in the assignment of a statistical value to medical certificates can be solved by reference to this book. To the perfection of this manual every registrar should contribute, for it is beyond the power of one man to make a complete concordance of the medical terms used in death certificates.

The question of state registration was brought up in the House of Representatives on April 23, 1902, by Mr. Russell, who introduced a joint resolution calling upon state authorities to coöperate with the census office in securing a uniform system of death registration. On the principle of one thing at a time and that done well, this resolution spoke of the registration of deaths only. The resolution was referred to the select committee on the census, and was returned with a favorable report, but amended so as to include the registration of births. It therefore appears that the statistics of life and death have at last reached equal rank with the statistics of coal and cotton in the esteem of the national legislature.

We have said so much about state registration because the subject is most inadequately treated in state laws, and because it is useless to hope that the practice of registration of deaths can be brought to anything like uniformity, or can fully serve the uses of preventive medicine, until all the people are brought within its scope. Some of our best registration states leave sparsely settled areas out of the account.

Naturally the value of registration is most appreciated in dense communities, and it is quite possible that the general practice of registration in cities has delayed the adoption of effective state laws applicable to rural populations. In such states as New York and Pennsylvania, where the great majority of the people live under local registration laws, the need of registration in rural and suburban districts seems perhaps not imperative, and besides there are practical difficulties in organizing a central registration office safely adjusted to a multitude of independent local offices. With the rapid growth of cities the relations of municipal sanitation to the hygiene of suburban and rural districts is coming more and more into view, and chiefly in respect to food and drink these relations are vital. Municipalities are everywhere clamoring against the villagers and rustics who abuse the watersheds, flavor the cabbage, lettuce, and celery with night-soil, ship infected milk to town, and sell the wornout cow to the city butcher. It is time that urban influences should combine to compel a strict and systematic accounting of deaths and the causes of death in the rural districts.

MUNICIPAL REGISTRATION.

In respect that the practice of registration in cities is more general and is rapidly spreading, and that municipal registration ordinances are usually effective, this phase of the subject is more encouraging. There is nevertheless somewhat to criticise. The worst faults of state registration grow out of neglect, but registration in cities is subject to positive abuses. The extreme diversity of methods and practice which we noted in state registration is less apparent in city registration, but in the mathematics of municipal registration we find vagaries of every sort, insomuch that the statistics of hardly two first class cities in the country are comparable, and many cities cannot safely compare their own records for different periods.

Popular interest in the vital statistics of cities has grown so far as to demand the publication in the daily press of yearly, monthly, and even weekly summaries, bringing their so-called rates into comparison with the similar statements of other cities, and with their own earlier statements. The confiding public assumes that the figures are as trustworthy as the market reports. The uninitiated citizen cannot be expected to know that the death rate is affected by several ever-active and variable actors besides the mortality; but he knows how to calculate interest, and he believes that the death rate is obtained by counting the deaths in a given period and dividing their sum by the population for the same period. It is a pity to disturb this ingenuous faith in municipal arithmetic, but the people ought to know how the death rates are obtained. The city registrar's arithmetic is seldom so simple. His permutations and combinations are perhaps not usually dishonest in intent, but the results are nevertheless misleading. If one will examine the death certificate of a city it will almost invariably be found that more space is devoted to the place of death than to any other single item. Certain apparently subsidiary data are required under this head, *e. g.*, "Place of former or usual residence;" "How long resident at place of death;" "If in a hospital or institution, give its name." These are proper inquiries. It is quite right that a great city should keep a separate account of the deaths which do not diminish her population, and almost all city registrars count off the deaths of transient visitors and of those who are brought to the city for medical treatment. There is no agreement among city registrars, and a wide diversity of practice is followed in solving the question between resident and nonresident. At the pleasure of the city registrar one may be counted out of the death register if he die within a week, or a month, or a year after coming into town. In a true registration area, where every death must be counted somewhere, the solution of this question may be effected with fairness and without appreciable vitiation of local statistics; for the proposal to exclude a death from record at one place invites consent to admit it to record at another. But in a nonregistration area there are not two parties to the question.

When on the other hand, bonafide residents of the city die outside the corporate limits, what shall become of the record? If the death happens in a nonregistration area and the body is not brought into the city, the occurrence never comes to the knowledge of the city registrar at all. If the body is brought into the city, the certificate is presented to the city registrar and the death may or may not go into the home mortality account, as the registrar decides. If he counts off the intraurban deaths of nonresidents he should, by parity of reasoning, count in the extraurban deaths of those who at the time of death were component units of the population. But under existing conditions the city registrar's own sense of honesty must direct him in this course. The matter lies in his own undivided confidence.

Most large cities maintain outside their corporate limits large institutions where dependent citizens are cared for, and among them a considerable annual mortality occurs. These extraurban deaths enter into the mortality accounts of some cities, but is that the practice in all large cities? One looks in vain through the health reports for information on this point.

But the number of the dead is only one factor of the death rate. The population is the other, and the modes of estimating population for intercensal years are likewise subject to curious variations. The statistical reports rarely contain a word of explanation on the subject. Some registrars take the difference between the results of the last two decennial enumerations and find its ratio to the population of the earlier year. One-tenth of it is said to be the ratio of annual increase over the population at the beginning of the new decennium. The assumption that the ratio of the last completed decennium will govern is perhaps reasonable, but the yearly ratio is not one-tenth of the decennial ratio, for the increment is compounded each year. One should find, therefore, what rate will yield the ten-year increment at compound interest, and apply this each year to his new principal.

Another plan of estimating the population is to multiply the number of children attending school in each successive year by a constant factor. In the same way the number of inhabited houses is sometimes used as a basis, the other factor being the average number of persons per house as determined by the preceding census. Again, the number of registered voters is multiplied by an assumed factor, the result being taken as the population for the year. In southern cities the voters are divided according to color, the number of colored voters being multiplied by a higher coefficient than is applied to the white voters. In conversation with city registrars I have learned that there are yet more curious modes of arriving at estimates of population; *e. g.*, to ask the publisher of a city directory or the inquiry department of a daily newspaper.

The first named method is the best, but every chance to correct it or to test it should be availed of, and when a new census shows that the estimates for intervening years were too large or too small, the corrections should be applied to all the ratios which are brought forward from year to year for purposes of comparison. It very seldom happens that official estimates of the population of large cities are too small, and the federal census has once or twice been almost persuaded that its own enumerations have been too large.

From what we have said it seems clear that one of the factors of a death rate is never exactly ascertainable and that the other factor is not the sum total of the deaths that occur in the population or in the political bounds of a city, but is a derivative of this sum total. Moreover it is derived not according to a fixed rule, but by various methods at the unchallenged and often inconstant discretion of individual registrars.

There is not in the country a single city which has not two mortality lists for every year, and a few, very few, of them publish an account of both lists. That an account of the deaths which do not enter into the rates is so seldom published seems rather significant. The people, who trust too much in death rates, and whose interest in the subject is far too little enlightened, have a right to know how the factors of the death rate are determined and what rate of increment is assumed in the population, and what proportion of the deaths happening in the geographic limits of the city and among its enumerated citizens has been considered to represent the net mortality justly chargeable upon the population. If the people have the right to know so much, still more are the 400 municipal registration offices entitled to this information, for if units of weights, volume and value should be familiar and of common use, no less should the balance sheets of life and death conform to a sealed standard.

Of the many cities whose published statements concerning mortality stagger one's faith, not all, possibly, are grossly erroneous, but whether right or wrong, all ought to display the elements of their computations. In every American city of 100,000 population whose claimed or imputed death rate is below 17 per 1,000, the rate has been obtained by some avoidable error such as (*a*) discounting too heavily the gross registration deaths on account of intraurban deaths of nonresidents; or (*b*) failure to debit the mortality account with the extraurban deaths of residents; or (*c*) overestimating the population; or (*d*) combining all or any two of the foregoing errors.

Or else the death rate is explained by (*e*) a peculiar age distribution which reduces the influence of infancy or of old age, or of both upon the gross mortality. The city may be too young to have acquired a considerable population above 60 or normal marriage and birth rates; or (*f*) with a normal sex distribution, the birth rate may be excessively low, the population being recruited chiefly by the immigration of young unmarried adults; or (*g*) a continuous high birth rate may have given the city an unusual proportion of population between 10 and 40 years of age; or (*h*) the low general death rate may be due in a normal population to an extremely light infant mortality.

It is just possible that one or two large municipalities may owe a death rate of 17 per 1,000 to natural advantages plus good sanitary administration, but without a single probable exception all death rates below 17 per 1,000, in cities 40 years or older, are delusions, and a percentage of them are statistical fictions.

In one or two large cities the official registrars have such skill in marshaling figures to prove what is untrue that one might infer equal ability to demonstrate the truth. But such an inference is not necessarily correct, for in vital statistics it is peculiarly difficult to avoid beginning an inquiry with a definite idea as to what the figures will show. Vital data are so complex and variable, and the processes are subject to so many corrections and interpolations that a rigid conscience and inflexible logic will hardly keep one true to the mathematical line. Statistics are to many minds highly intoxicating, and some registrars seem to be habitually exhilarated.

REGISTRATION OF BIRTHS.

If we have so far made out our case for the complete, uniform, and systematic registrations of deaths, and for temperate and logical utilization of the accumulated data, we must at the same time have shown that statistics of death alone will not under any circumstances suffice to indicate the sanitary or vital conditions of the people, for all the ratios derived from the death returns vary from time to time and from place to place, in obedience to influences which have little or nothing to do with sanitary administration. To analyze death records according to cause, age, sex, season, locality, race, occupation, social condition, and to apply the numerical results to a living population which has not undergone a similar analysis for the purpose of ascertaining the vital conditions of the people, is about as logical as if one should undertake to estimate the motive power of a great railroad system by a complete and exhaustive examination of the scrap heap. Without knowledge of the sources of population, the mortality statistics can never be made to yield the information which the sanitarian most needs and which, if one may judge by the popular appetite for statistical hokey pokey, the people most demand. This does not belittle the very large independent value of mortality statistics, but emphasizes their superior importance as an articulated part of a comprehensive and logical plan of accounting in the vast business which we call government. Public hygiene has long outgrown the sanitary bookkeeper; its needs exceed the capacity of the expert accountant, and its affairs can only be reduced to precise order by the highly developed skill and perfect grasp of the actuary.

All the rates derived from the mortality returns are properly called crude ratios, and should be corrected by all the determinable causes of variation. It is an idle saying that the variable minor factors neutralize each other. Some sources of error may be negligible, but none should, without test, be neglected. Of first importance in this connection is the birth rate, which is not in any degree less important than the death rate. The private interest of the citizen in registration of births is indeed superior to his interest in registration of deaths, for a great proportion of his privileges and immunities, rights and duties, turning upon the question of his age and his parentage, are definitely conserved by the registration of his birth.

The defectiveness of American statistics of birth is so great that outside of New England probably not a state or city can determine a fairly approximate birth rate for any year. Many states and all the registration cities have more or less ineffective laws upon the subject. In some places the records of births are obtained by annual or semi-annual enumerations, but this method is no more successful in the registration of births than in the registration of deaths. Chapin says that in Providence skilled enumerators obtain by semi-annual enumeration records of but little more than 60 per cent. of the births. Local laws usually require births to be reported by the attending midwives or physicians, but the time limit for making the return is usually very liberal, and penalties for neglect are either not provided or else never enforced, so that the results are always far short of the true number. Even in those states where the legal provisions are most effective it is found necessary to supplement the returns by enumeration and by consulting the church registries of baptism.

In the registration of births it has always been found expedient to consider the confidential relations of physicians and midwives, and the private interests of parents, not only in respect of illegitimacy, but also with regard to the date and even the place of birth. Leniency upon these points opens the door for lax practice in other respects. The element of compulsion enters practically not at all into American practice, and perhaps there is no place for it. In respect to a birth the state cannot compel, as it can in regard to a death, the testimony of any witness.

The key to a complete registration of all legitimate living births is the conditioning of future rights and privileges upon the record of births, as is done in Germany; but it is doubtful if this could be done anywhere in America. A certain degree of pressure could, however, be brought to bear upon the subject, and the means are already available in the laws. For instance, the right to attend the public schools and the right to vote both depend upon attained age. If it were necessary to accompany every application for either of these privileges by some sort of signed testimony as to the age of the applicant, a copy of the birth certificate would offer the simplest means of meeting such a requirement. Thus gradually the birth certificate might come into habitual, though not exclusive or enforced, use in establishing citizenship. Many difficulties undoubtedly attend the registration of births, but satisfactory results have long been obtained in England, Germany, and France, and in at least one American state—Massachusetts.

Among the various methods of obtaining birth records we may speak of those employed in Massachusetts as well conformed to our republican ideas and as the most successful. Physicians and midwives are required to report monthly the births occurring under their care. Parents also must report in 40 days. In order to keep the physicians reminded of this duty a ruled sheet 4 inches by 11 inches in size, is mailed each month to every physician with a request for a list of the births attended in the preceding month. This sheet is divided into ten vertical volumes for the ten required items of information, so that the record of each birth occupies one line across the sheets. The data provided for on the sheet are the date of birth, sex, color, name, place of birth, name of parents (mother's maiden name), residence of parents, father's occupation, birthplace of father and mother.

These record sheets hardly meet the private needs of the citizen, being inconveniently preserved and difficult of reference, but they do serve the purpose of vital statistics with which we are at present concerned. Once a year an enumeration of births is made, and the returns of parents, physicians and midwives are thus supplemented. It is also found advisable to consult the pastors of certain religious bodies in order to get the records of births occurring among foreign-born people under the care of foreign midwives.

In some places, as in Cincinnati, the sanitary police call once a month upon clergymen, physicians, and midwives for their returns of births. The success of such visitations depends upon finding the certifier at home and at leisure, so that the usefulness of this plan, except in a supplementary way, is doubtful.

A difficulty which often places a wide interval of time between the birth and its record is the item of Christian name. Now, the name of the child has no statistical importance, and, provided that the right to a completed record is secured to the new citizen, there is no reason for delaying the report for want of that item. Indeed, the experience of my own state seems to indicate that parents are easily impressed with the importance of sending in this final bit of information, and in about 65 per cent. of instances do complete the record by sending in the name.

The data required upon a record of birth for statistical purposes do not all appear upon the Massachusetts blank. Thus the ages of the parents should be given, and the number of the children (first, second, third). Items of doubtful value or of merely curious interest are sometimes asked for. In some certificates the hour of birth is asked for. In Wisconsin the certificate asks for the day of the week and for the names of the other living children of the same parents. In contrast with Indiana's impious pursuit of a death record by means of a mattock and spade, the birth certificate of the city of Mobile meets the unborn babe with an impudent inquiry; "*What part presented?*" There is no room on a birth record for any inquiry which is not of definite use to the citizen or to the community, and the difficulty of obtaining careful records increases in proportion to the number of separate inquiries. Thirteen items suffice for all the legitimate uses of a birth certificate.

By bringing several sources of information under tribute, and employing two or three agencies of collection, restricting the scope of each inquiry to the unquestionable needs of the public, and by keeping always in the foreground the private and personal interest of the citizen in the registration of births, it should be possible to realize in any American community a registration of births as complete as is obtained in Massachusetts. When several sources of information are interrogated especial care must be taken to avoid duplication of returns, but any good plan of registration will always include the supervision of the returns by a special officer, who will take whatever pains are necessary to find out whether two records which resemble each other belong to one event or two.

We have alluded in our discussion of mortality statistics to some of the more important uses of birth statistics, and shall mention here briefly some other sorts of information derivable from the study of birth records. As the birth rate has its measurable effect upon the death rate, so the death rate has at times an appreciable effect upon the birth rate. Birth rates in general obey the marriage rate, which always rises in times of prosperity; but when the infant mortality is high the fecundity of marriage is raised by abbreviation of the suckling period, so that the explanation of a marked rise in the birth rate for a given year may be found in the statistics of the previous year, and not in the marriage rate, but in the infant mortality.

The effects of a high birth rate during a period of years are to be looked for not only in an increased gross death rate for approximately the same period, but may sometimes be traced to a diminished death rate at a much later period when the growing children have made unusual additions to that part of the population which furnishes the least mortality (10 to 40). In the same way a period of high reproductiveness leads to an increase of the relative proportion of women of the child-bearing age at a later period, so producing a second wave of reproduction.

War, by taking away vigorous young men, depresses the marriage rate and the birth rate, abstracting at the same time from the home population those lives which contribute least to the mortality rates. After the return of peace this loss is not compensated, for war has its evil influence not only upon the lives of those engaged but their yirility and upon their inclination and ability to marry.

Prosperity increases the marriage rate, but not, in corresponding degree, the birth rate. The birth rate is generally higher among the poor, and as families rise in the social scale the production of offspring tends to be more and more controlled. This condition is now generally observed throughout the civilized world and is not without serious significance. Long continued prosperity brings a decline of the marriage rate with a greater average age at marriage, thus further depreciating the reproductive energy of the people. Nations as well as men may therefore find sweet profitableness in the uses of adversity.

The annual rhythm of fecundity which has sometimes been noted is quite curious. If constantly looked for it perhaps might be often observed. Thus in the experience of Hamburg for ten years (1881-1890) the months of February and March gave the highest percentage of births, while June was the month of lowest percentage. A similar observation in Berlin for 1895 showed that January, February, and March gave the highest percentage, November the lowest. The birth rate is also influenced by density of population, by divorce, by race, and by religious belief.

Still-birth and illegitimacy bear quite irregular relations to the general birth rate. Inded the still-births appear to obey the illegitimate rather than the general birth rate. Illegitimacy does not, as might be expected, rise as the average age at marriage increases. On the contrary, in England, with the fall of marriage under age, illegitimacy has also declined.

Still-births have not received due recognition in registration. The eleventh census included still-births in the mortality and so charged against the population the deaths of those who were never in any sense a part of the population. In the comparative rates published by twelfth census the figures of the eleventh census have been corrected for still-births. In England still-births are not recorded at all. In France and Belgium all children who die before their births are reported are registered as still-born. The birth rate for these countries must therefore be corrected before they become comparable to the birth rates of Germany and Italy, where only the infants delivered in the last two months of gestation, and dead without having breathed, are registered as still-births. The birth rate may for certain purposes be stated, as the death rate is, in ratio to 1,000 living population, but such a ratio is crude in a more obvious way than our customary crude death rate.

The ratio of births to the number of living females of child-bearing age is a more generally useful form of statement. Where populations differing materially in social conditions are to be compared the ratio of legitimate births to married women of child-bearing age must be determined.

STATISTICS OF MARRIAGE.

The registration of marriage is quite generally and effectively provided for in this country, 45 states having laws upon the subject. In all the states license must be obtained before the ceremony is performed, but return of the fact of marriage is not provided for in the laws of four or five states. In the New England states the license itself becomes a certificate. The items of information noted in the license suffice for the needs of vital statistics, but the records are often in the form of stubs, and are not generally available for the uses of vital statisticians. Provision for the return of the event has usually been made no stronger than will meet the private interests of the contracting parties, so that the records of marriages are not so complete as they would be if the interests of the state were fairly secured by the laws of the subject. Still, tolerably effective machinery is provided for the collection of statistics of marriage and it is only necessary to direct our present plea to the better utilization of existing data. It is highly desirable that returns of marriage with all their items of information should be brought, if not under medical supervision, at least into the hygienist's general store of vital memoranda.

STATISTICS OF SICKNESS.

When the fact that crude death rates are comparatively unmeaning has been firmly grasped, and when we have realized the need of detailed information concerning the composition and movements of population, the sources of their natural and their actual increase, and the influences which lead to death and decay, we shall find that the productive energy of the people is not more closely related to the chief events of individual history (birth, marriage, and death) than to a multitude of minor occurrences which are more or less disabling.

Perhaps the most dangerous fallacy in the use of mortality statistics arises from the assumption that the mortality obeys the morbidity, and that the prevalence of sickness may therefore be approximately estimated from the recorded mortality. The two curves do indeed correspond in a rough way, but the intervals are so variable that different statisticians have furnished coefficients as far apart as three and nine, for the purpose of estimating general morbidity from general mortality.

Long ago an eminent English actuary, Mr. Neison, reduced this assumption to an absurdity by showing, from the records of medical charities, that in London in a year of favorable mortality there were no less, probably more, than 35 cases of illness to each death. Neison also showed that in 16 selected occupations, whose mortality was low, the sickness rate was high; while in other occupations, whose sickness rate was low, the mortality was high. He also worked out a table of ratios of mortality to sickness by age periods, finding the lowest 1.5 per cent., in the age period 10 to 15, and the highest ratio 62.5 per cent., in the age period 85 to 90; so that age distribution plays as important a part in the disability rates as in the death rates.

Rumsey found that "deaths in a younger population are associated with less sickness than deaths in an older—in a male population than in a female—in an agricultural than in a manufacturing population." Newsholme testifies to the same effect, and more vigorously: "It is fallacious to assume any fixed ratio between sickness and mortality. Death returns are silent about the large mass of common sickness, which, although it may disable a man, is not 'unto death.' From an economic point of view this sickness is *more important* than deaths."

It has always been cheaper to bury a dead man than to support a sick one, and the contingent and remote effects of sickness have always been of more practical concern to the sanitarian than the number and causes of death. This view is very ancient, and has always had a strong political influence. The most convincing argument which Chadwick was able to urge in his movement for a general registration law in 1837, was that the cost of sickness was the heaviest item in the poor rates.

Registration of all sickness, or even of all disabling sickness, is impossible. The boundaries between health and disease are exceedingly vague, and the nature of much disabling sickness is indeterminate. But, because the registration of all disabling sickness is impossible, it does not follow that we should be content with the small amount of information which the notification laws afford. These laws attempt to cover only the more dangerous communicable diseases, the special agents of untimely death. The presumed intent of such laws to limit preventive operations to these diseases is by no means acquiesced in by the practical hygienist, and no small stir has been created now and again by attempts to include within the scope of the notification laws diseases whose infectious nature have been more recently recognized.

The efforts of preventive medicine must, in general, be directed against those diseases which cause the highest mortality, and every other account whose settlement we may or may not be able again to postpone. The winnings of hygiene have not as yet probably altered the age constitution of living populations to a measurable degree, or brought clearly into view the increased power of those tax-gatherers into whose hands our disputed accounts have fallen. But it is certain that what we win in one age period, or against one agent of death, we shall have to contend for again in a later period and against another agent, so that we must constantly readjust ourselves to shifting phases of the contest. Advance information will enable us to do this promptly, and such information is to be sought, not in the mortality returns, but in returns of sickness.

Of several attempts to collect statistics of sickness in this country, but one has survived a considerable period and yielded approximately uniform results. The Michigan weekly returns of prevailing sickness are sent in by a number (usually about a hundred) of voluntary observers in various parts of the state. They report simply the presence or absence of certain specified diseases, without any details as to the amount or intensity of such sickness as is observed. This work has been kept up for 25 years. While this plan furnishes but a small part of the information needed by the sanitarians, the accumulated data have, nevertheless, a surprising accuracy, and would seem to indicate that the assay method of estimating the amount and kind of sickness may be satisfactorily applied to such data as are already in many places available.

The establishment in connection with boards of health of bacteriological laboratories furnishing physicians, free of cost, valuable assistance in the diagnosis of certain infectious diseases, has brought public hygienists into possession of amounts and sorts of information which could not be obtained under notification laws. Born of necessity, this movement has made astonishing progress. Within a decade twelve or more such laboratories have been established under the control of as many state boards ot health, thus making small communities and rural districts contributors to the statistics of sickness. There are besides fifty or more such laboratories in connection with city boards of health, and in some of them the number of records concerning separate cases of disease mount well up in the thousands each year. Besides the definite information furnished by these reports, whose immediate use has been of surpassing value, the accumulated data will doubtless yield under statistical treatment results hardly less profitable.

With the advent of the public laboratory a new bond of mutual helpfulness has been created between public health officers and practising physicians, and the advantage of such a fortunate circumstance should not be lost. Since every man is supposed to contribute to the support of the commonwealth, there is a natural inclination on one hand to dispense, and on the other to accept, the benefits of the public health laboratory as if the state's whole concern in the matter were limited to the need of the moment. One has already heard practising physicians declare that the city or state is not entitled to the information asked for on laboratory blanks, and some physicians endeavor to evade the manifestly proper inquiry as to street address. What is worse, health officers seem inclined to condone such defaults. This is to lose the smaller but cumulative profits which the state should realize upon such an investment. It fosters, besides, a light esteem of professional privilege which will in time take on the characteristics of parasitism. The commonwealth is well within its right in requiring of every physician, who avails himself of the proffered aid, careful attention to the details of information which are provided for on the clinical and laboratory blanks.

Of the total amount of sickness occurring in any city, 40 per cent. at least is treated wholly or in part at public expense, and it has often been proposed to make the records of such sickness available for statistical uses. Hospitals and dispensaries have always manifested a decided opposition to such propositions, and have always been able to defeat them. It is nevertheless perfectly just and proper to make statistical use of the records of all sickness treated at public cost, and while municipal registration offices do not anywhere come into possession of such data, many hospitals and dispensaries do at the present time report at definite intervals both the kind and amount of sickness coming under treatment at the charge of the municipality. Some of the data required for sanitary purposes are not found in these reports, but the missing items are usually on record either in the hospital memoranda, or at the police station, or with the boards of charities. To obtain at least the temporary use of information from such sources does not, therefore, involve any increase of clerical work at hospital or dispensary, but requires simply a modified form of municipal accounting, which could often be secured by mere consultation and at no considerable expense.

Another new arm of municipal hygiene, which adds much and might add more to the statistics of sickness, is medical inspection of schools. In Boston, where this practice is the oldest (since 1894), the returns for a year include upward of 10,000 cases of sickness falling under 100 or more nosologic heads. The inspectors' reports are made monthly to the board of health. In New York the inspectors report to the board of health daily. If the teachers were instructed to supplement the inspectors' reports with the results of inquiry concerning absenteeism a more satisfactory view would be obtained of the sickness prevailing among children.

By easy arrangements boards of health might secure the temporary use, if not the permanent possession, of all needful data concerning the causes of disability among a very large part of the population, and timely needs would be served in this way. But, as we have already seen, the details of present sanitary history are sure to be needed again and again in tracing the causes of remote events, and for such purposes they must come complete into the permanent possession of boards of health. Never was a day so auspicious for the acquiring of such property. In no aspect of modern medicine is the scientific spirit more strikingly manifest than in the completeness of medical anamnesis. It is an age of exhaustive histories, of systematic and minute note-taking, of graphic memoranda, of indexes and cross-indexes. The few and simple data required for the broader purposes of preventive medicine need only to be abstracted from heaps of manuscript, and so far as these relate to sickness treated at public cost, their appropriation to the service of preventive medicine needs no apology. On the contrary, failure to so appropriate them must inevitably become a reproach both to medical charity and to municipal government.

A comprehensive plan of collecting statistics would include reports of sick benefit associations and the disability returns of corporations, such as mining, transportation, and industrial concerns. In Egland the factory and workshops act furnishes a great deal of useful knowledge concerning industrial hygiene, and in Germany the Allgemeine Krankenkassen make regular reports to the health officials. In time such sources of information may be brought under tribute in America. Farr's original plan was yet more ambitious. He confessed an ultimate aim to register all the sickness among the civil population as completely as is done in the army. Our own remote ideal may be as great as that, but meanwhile our plea is for the instant seizure and conversion to the uses of hygiene of every scrap of information coming within official reach.

SUMMARY.

The natural sciences have contributed to the welfare of mankind no gifts more beneficent than those which are appropriated to medical uses. Preventive medicine, in particular, has been vastly enriched, insomuch that we seem in danger of forgetting that resources so great need to be economically administered. But even unlimited means cannot, by mere distribution, either reach the greatest number of beneficiaries or confer the greatest benefit so far as they reach.

The profits of public hygiene are realized no more in individual safety and comfort than in improved business and social conditions. It is in the latter aspect that the benefits of public hygiene are most striking and have greatest influence upon appropriations of public money to sanitary work. Moreover, the agencies themselves, whose employment leads to such results, need both the highest form of certification for their present performances and the surest actuarial basis for their future enterprises.

As certainly as the problems of transportation wait at present upon the perfection of roadmaking rather than upon improvement of rolling stock or motive power, so certainly do the "giants of the hour," the natural sciences tributary to preventive medicine, wait upon a correct and economical administration of the resources of public hygiene.

The urgent need of the moment is a thorough and comprehensive plan of accounting for vital memoranda. The sources of essential information are, speaking broadly, two, viz.:

The federal census, which, being now maintained as a permanent part of the national government, should (*a*) be, at the approach of another census year, better than ever prepared rapidly and accurately to count the people, making notes of all the data needed for statistical purposes; (*b*) should make such *ad interim* studies of the births and deaths occurring in nonregistration areas as will serve for the better correction of the enumerators' returns for the census year; (*c*) should commit all vital returns to the hands of a trained medical statistician who understands the moods and tenses of vital mathematics.

Local registration under state laws, embracing in their operation all the people, rural as well as urban; with substantial agreement among the states as to the essential data of record; respecting fully the rights of registration cities to the custody of their own records; but securing absolute unity of method in statistical treatment, with an open display of both gross and net mortality and of the means of arriving at population estimates.

Local registration must cover: (*a*) Records of deaths made at the time and place of their occurrence, by the most competent persons acquired with the facts, including a medical certificate of the cause of death, the making and filing of such a record being in every instance an indispensable preliminary to the disposal of the dead body; (*b*) records of birth, securing by the payment of fees, by every appeal to private interest and public necessity, including, if possible, conditioning of certain privileges of citzenshp upon recorded evdence of attained age; (*c*) records of marriage and divorce, with all such items of information as have statistical importance; (*d*) records of sickness, including all cases of those infectious diseases which fall within the provisions of the notification laws, all cases of sickness which come under study in the public health laboratories, all sickness which is relieved at public cost, all sickness falling under the observation of inspectors of schools, tenements and factories, and all sickness occurring in the public services, *e. g.*, army, navy, police and fire departments.

Finally, the data of vital statistics should be systematically utilized, not only for the broader purposes which have grown into common use, but for the minuter inquiries which such records may answer.

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CO-OPERATION ESSENTIAL TO PROGRESS IN VITAL STATISTICS.

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This paper is an attempt to show some of the ways in which coöperation between the national, state and municipal offices engaged in the collection, tabulation and study of vital statistics may be very useful or even indispensable to further progress.

Vital statistics, and especially mortality statistics, are universally acknowledged to be the absolutely necessary foundations of modern public-health work. Notwithstanding this fact, and although scarcely a state or city in the country is without its public health administration, the United States is almost alone among the civilized nations of the world in having no national system of vital statistics.

This condition has arisen from the constitution of our government, whereby the collection of vital statistics has been usually a part of the state administration, although in some cases data have been obtained by city ordinances without correlation with the state administration. The function of the national government has been confined to an effort to obtain vital statistics in connection with the regular decennial censuses, at first by an attempted enumeration of the facts coincident with the numeration of population and latterly by an increasing use of the material collected by the states and cities comprising the "registration area."

The statistics collected by the individual states and cities have varied greatly in quality. The most diverse methods of collection and compilation have been employed, and only a small proportion of the states that have attempted to collect such data have made a practical success of it, even on the somewhat low standard of 90 per cent. of accuracy for mortality statistics alone. There has been little coördination, and a state undertaking to install a new system of vital statistics has been quite as likely to make all of the blunders that have involved other states in disaster as to follow the methods that have been tested by experience in other states and found essential to satisfactory results.

After the statistics have been collected, the same hopeless lack of uniformity and approved system may be found in the registration reports published. Tables regarded as essential in the reports of one state may not be represented at all in those of another, and the details of age periods, etc., vary so greatly that comparisons are frequently difficult or impossible. No uniform methods of computing and stating death rates are in general use, and up to a very few years ago no general agreement existed in even such a vital feature of mortality statistics as the adoption of a uniform classification of causes of death.

The latter difficulty—the adoption of a uniform classification of causes of death—stood at the very threshold of any course of action whereby vital statistics might be made more uniform and comparable than formerly. If registration officials could agree on the use of a uniform and fairly satisfactory classification, it was likely that they would afterwards come to a basis of satisfactory union with respect to other statistical features of scarcely less importance. But for over fifty years the lack of a uniform classification of causes of death had stood in the way of progress in vital statistics, and it seemed hopeless to attempt much in this direction until this obstacle could be removed.

As you know, the American Public Health Association was largely instrumental in bringing about this greatly needed reform. At the meeting held at Ottawa in 1898 the Association urgently recommended the adoption of the international classification, as it is now known, by all of the registration offices of the three countries, and especially by their census offices. The general plan of a decennial revision proposed by this Association was indorsed by the International Statistical Institute, the revision was held at Paris in 1900, a special invitation being extended by the French government, through the usual diplomatic channels, to all of the countries participating, and the results were presented for general use beginning with the first year of the present century, 1901.

All of the registration states of the Union have adopted this uniform system, as well as the United States department of labor and the United States census. The classification is likewise supreme in Canada and Mexico, so that the constituent countries of this Association are a unit in this respect. It is proper to say that the preliminary work of revision was not as completely carried out in this country as it should have been, owing to the fact that the recommendations of your committee on demography and of your special commission on the revision of the Bertillon classification made to you at the meeting at Minneapolis in 1899 were not carried out, owing, perhaps, to the late period of the session at which the report was made. As far as possible, under the circumstances, the United States commission of revision performed the necessary work up to that time and all of the suggestions received from American registrars were given consideration at Paris. No invitation to the international commission of revision, however, reached this Association or its special commission, by whom all of the preliminary work in this country had been done. The United States was represented by the United States marine hospital service, which had not previously been associated in this work except as represented by delegates to this Association. For this reason the commission and the Association may disclaim responsibility for the actual results of the revision, although it seems desirable that we now heartily join in the use of the revised version, until an opportunity shall come with the next regular decennial revision, to further perfect the work.

As an indispensable step in this direction, the United States census office has recently issued a manual of international classification which contains not only the assignment of all terms found in the French version but also many hundreds of terms actually occurring in the practice of American registrars, but for which no provision had been made by the international commission. Some of the more important of these terms would have been regularly provided for had not this Association failed to authorize the completion of the work undertaken by it, but as the exigencies of the census compilation would not permit delay or reference to committees, the assignments of all such terms were necessarily made with the hope that they would prove, for the most part, in accordance with the general judgment of American registrars.

Another matter of great practical difficulty in the compilation of mortality statistics is the treatment of returns in which physicians have assigned two or more causes of death for the same case. The international commission did not consider this subject in detail, although a method of procedure was prepared by Dr. Bertillon and appended to their report. Many of the decisions, however, would hardly be acceptable to American registrars, and the whole subject demands the most earnest attention in order that some rational, definite and practical plan of treatment be formulated and generally adopted.

It may be feasible to introduce a system in which certain weights or "ratings" shall be assigned to each cause of death as reported by physicians. We ordinarily consider two factors in determining the precedence of one cause of death over another, in the absence of any other information as to duration, primary or secondary occurrence, etc. These factors are (1) the *importance* of the title to which the given term would be assigned in the classification, and (2) the degree of certainty that the term really belongs, under that particular title. Thus, using a decimal scale, pulmonary tuberculosis is a title of first rank, say 10, as compared with pulmonary congestion, say 2 or 3 on the same scale. If chronic Bright's disease be returned in connection with pulmonary tuberculosis, we can compare the two by assigning the same number to Bright's disease if we consider it of equal statistical importance, or by assigning a lower number on the scale, as 8 or 9, if we consider it less important. Again, whatever rank we give to Bright's disease, it is evident that a death so returned is much more certainly due to the disease in question than if returned from uremia, which is likewise compared under this title, perhaps improperly. Uremia should be marked, 1, 2 or 3, with respect to its validity as a synonym of Bright's disease. Now the products of the numbers representing the *importance* of the titles into the numbers representing the *validity* of each term under its title, both taken on the easily decimal scale, will give an index number for each term on a scale of 100 by which the preference of assignment between any two or more independent terms may be decided. The occurrence of diseases as frequent complications or sequelæ would be taken into consideration in a general way in rating the relative importance of the titles, besides which complete lists should be prepared showing such relations.

Such a plan as that suggested for the proper disposition of jointly returned causes of death would not only be of service in determining such assignments, but it is evident that a series of rates obtained for this purpose would be directly in the line of a detailed and specific criticism of the classification itself. Should a given title be generally rated as of "0" importance, or a certain term be generally marked as not properly pertaining to the title under which it has been included, then we would have exactly the information that we require for proposing and supporting a change in the international classification. In other words we would substitute a definite numerical rating for a more general expression of opinion.

To make the proposed plan a success it is necessary that the fullest coöperation should obtain between all of the important interests represented. Neither the collectors, compilers or users of mortality statistics should be neglected, nor, above all, the wishes of the mass of the profession from whose returns of causes of death all our mortality statistics are derived. It is a large undertaking and calls for careful planning and excellent facilities to make it a success. By the use of the manual of the international classification as a basis, and with the full coöperation of the census authorities with committees representing this Association, the Conference of State and Provincial Boards of Health of North America, the American Medical Association, and the various special organizations, I am confident that a thoroughly representative body of opinion could be collected in useable form, and I hope that this Association may decide to undertake the work. I regard it as not less important than the resolutions passed by this Association in 1898, which were the first harbingers of international uniformity.

The Conference of State and Provincial Boards of Health has already constituted a committee, by action taken at its recent meeting at New Haven, October 28-29, which is fully authorized to act in conjunction with the committee of this Association for the improvement of methods in vital statistics. The text of the resolution under which the committee of the Conference was appointed read as follows:

> *Resolved,* That a Committee be appointed by the Conference of State and Provincial Boards of Health of North America to coöperate with the U. S. census and with similar committees from the American Public Health Association, American Medical Association, and other organizations, in the promotion of the uniformity and comparability of mortality and other vital statistics and their satisfactory presentation in statistical reports and bulletins.

It will be observed, under the foregoing resolution, that the committee of the Conference proposes not only to deal with matters of classification of causes of death, but also with the methods of statistical compilation and presentation of mortality and other vital statistics in the various annual reports and quarterly, monthly or weekly bulletins. It is desirable that important tables be always given in these reports in such forms that comparisons will be readily made, and it is possible that study of the subject will show that certain tables may be represented in better forms and other tables, possibly, be dispensed with in favor of more important data. Rates should be uniformly calculated, and reliable methods should be employed for estimating populations for intercensal years. The Unitel States census office, which is now established upon a permanent basis, has in preparation the tables for the first of the regular annual reports on mortality statistics derived from the registration areas of the United States, and as the material for these annual reports consists of transcripts of the records of the state and municipal registration offices, it may be desirable to avoid unnecessary duplication of work by utilizing certain of the standard tables of the census reports.

We have now in the census office a clearing-house in which the mortality statistics of all of the registration states and cities will be regularly brought together each year for statistical study and comparison. As far as it goes, and it is altogether dependent upon the extension of reliable methods of registration by states and cities, it constitutes the national system of registration that we have all so long desired for the United States. It has the disadvantage, from a practical point of view, that it cannot deal at first hand with the collection of the data, but must depend upon the efficiency of the various state systems of registration. It is, therefore, to us, the members of this Association concerned with the administration of state registration laws, that the census must look for the quality of returns, and we owe it to ourselves as constituent parts of the national registration system that the character of these returns be made as excellent as possible for each of our individual offices.

While the census does not undertake to collect vital statisics directly, nor to enforce any regulations under which such work must be conducted, it is eminently proper that it should assist state officials undertaking such work or desiring to adopt better methods and endeavor to insure that the results obtained shall be sufficiently uniform and accurate to fit them for national use. To this end the special circular on the essential requirements of a registration law was prepared in coöperation with a committee of this Association, one of whose results have been the adoption of a standard form of certificate of death in Colorado, District of Columbia, Illinois, Indiana, Michigan and New York, representing an aggregate population of 17,846,306, or about 70 per cent. of the present possible registration state population. In the preparation of the manual of the international classification, as well as the suggested work in regard to uniform methods of presenting mortality statistics, the census has earnestly endeavored to carry out the expressed wishes of American registrars and sanitarians.

The only direct leverage that the census at present possesses with reference to the extension of satisfactory methods of registration is the power that it has to decide whether a state or city shall be included in the registration area or not. Certainly a great state like Ohio, or Pennsylvania, upon whose statute books registration laws have been written for years past, should take shame that such legislation is so inefficient that the state was not admitted as a registration state by the census. And a state like Delaware, once admited but now dropped on account of inaccuracy of returns, should endeavor to retrieve the lost ground and once more have the privilege and honor of belonging to the registration group. I assure you that we appreciate that privilege in Michigan, and I believe that the whole state takes pride in the fact that it was the first state of the West to be admitted to an equal standing with the progressive registration states of New England, New York and New Jersey.

Besides the advantage of belonging to the registration area, and I hope that members of state and local boards of health here present will not desist from active work until proper laws have been enacted and enforced in their states relative to collection of vital statistics, there is another way in which it might be practicable for the national and state services to coöperate, providing that congress could be brought to see the desirability of such legislation. As furnishing the primary data upon which the national statistics of the census office are based, the several state registration offices are an integral part of the national system. The states pay a considerable sum each year for the collection of deaths, at the rate of twenty-five cents or more apiece for each death registered. The deaths thus returned are freely placed at the disposition of the government without compensation except a nominal amount for the cost of making transcripts. Without proposing any direct compensation to the states for these returns, except such as at some future time may be necessary in certain localities for the purpose of insuring complete registration, I would suggest that it would be proper and desirable, and would greatly facilitate the collection of vital statistics to extend the facilities of the United States postal service to the collection of vital statistics by the states. Just as in Canada, all returns of deaths from the local registrars to the central registration offices of the state should be sent postfree, as well as the returns from the state offices to the census office at Washington as at present. The use of blanks for securing corrections of imperfect returns from local registrars or from the attending physicians would be greatly extended, and the general supervision and control resulting from the necessary maintenance of a certain standard of efficiency by the state offices in order to enjoy these privileges would tend to elevate the entire service. In no other way would it seem that congress could so effectively and yet unobtrusively exert an influence for the extension of satisfactory registration methods in this country as by offering the free use of the mails as a direct inducement to attain a certain minimum standard of efficient registration.

Coöperation is the key to success in this work, which is dependent upon the harmonious action of so many disconnected offices and interests. The benefits and obligations should be mutual as between the local and municipal offices and the central registration office of the state and between the latter and the national registration office. The people should support the registration work, and all registrars, public health workers and members of the medical profession should educate the people as to the necessity for the collection of vital data. Local registrars should endeavor to raise the standard of returns of causes of death and should depend upon the hearty coöperation of the profession for this end, supported by assistance from the state office. The latter should endeavor to make its returns uniform, prompt and accurate, so that they will be fully available for the use of the annual reports of the census. In its turn, the census office should endeavor to aid in the spread of registration in states that do not possess such laws at present, should furnish all helpful aids in its power to the state and local offices, and by its annual reports embracing returns from all registration areas will be enabled to make the mortality statistics received of the greatest practical value to registrars and sanitarians. Finally, the function of such organizations as the American Public Health Association is a very important one, for by their agency the necessary organization and helpful coöperation between all the interests and agencies involved can probably best be effected.

We are at the beginning of a new era when for the first time in the history of this country there is an opportunity for continued systematic coöperation between a permanently organized department of the national government and the various state and municipal systems of registration to the end that accurate and uniform statistics of mortality may be obtained for the United States and made available for the use of all public health offices. This Association has already taken the initiative in securing a greater degree of uniformity in the tabulation of mortality statistics than has ever existed in the history of the country or of the world, and has further most efficiently coöperated with the permanent census office in efforts for the promotion of the satisfactory registration of deaths. Let the success of our previous efforts stimulate us to increased activity in this direction, and by enlisting the aid of the various national and state associations that may be prepared to coöperate with us and with the Federal government, let us endeavor to further promote the usefulness of our vital statistics and enable the United States to take a leading place in this respect among the nations of the world.

JOINT RESOLUTION

REQUESTING STATE AUTHORITIES TO CO-OPERATE WITH CENSUS OFFICE IN SECURING A UNIFORM SYSTEM OF DEATH REGISTRATION.

WHEREAS, The registration of deaths at the time of their occurrence furnishes official record information of much value to individuals; and

WHEREAS, The registration of *births and* deaths, with information upon certain points, is essential to the progress of medical and sanitary science in preventing and restricting disease and in devising and applying remedial agencies; and

WHEREAS, All of the principal countries of the civilized world recognize the necessity for such registration and enforce the same by general laws; and

WHEREAS, Registration in the United States is now confined to a few States, as a whole, and the larger cities, under local laws and ordinances which differ widely in their requirements; and

WHEREAS, It is most important that registration should be conducted under laws that will insure a practical uniformity in the character and amount of information available from the records; and

WHEREAS, The American Public Health Association and the United States Census Office are now co-operating in an effort to extend the benefits of registration and to promote its efficiency by indicating the essential requirements of legislative enactments designed to secure the proper registration of all deaths *and births* and the collection of accurate *vital* statistics, to be presented to the attention of the legislative authorities in non-registration states, with the suggestion that such legislation be adopted; now, therefore,

Resolved by the Senate and House of Representatives of the United States of America in Congress assembled, That the Senate and House of Representatives of the United States hereby expresses approval of this movement and requests the favorable consideration and action of the state authorities, to the end that the United States may attain a complete and uniform system of registration.

REPORT OF COMMITTEE ON DISINFECTION AND DISINFECTANTS.

BY PROF. F. C. ROBINSON, CHAIRMAN, BRUNSWICK, ME.

A committee of the American Public Health Association has, as we understand it, two prominent functions and duties, to personally conduct experiments bearing upon the topic assigned to it and bring the results to these meetings, and to find out what others are doing in the same line and make their results available. It generally happens that the members of a committee are busy men, fully occupied in the routine work of their professions, and hence have little time for original work. Such is pre-eminently the case with this one. But we are glad to report that in spite of this several of them have done valuable experimental work, and a part of this will be presented in separate papers.

These papers and their discussion will undoubtedly bring out better than any general report can the present condition and recent progress of disinfection, and we wish to give as much as possible of the time alloted to the committee to them.

As to what has been done by persons outside of the committee we can say briefly that a careful examination of articles published both in America and Europe has failed to reveal the discovery of any new disinfectant or process of disinfection of great practical value to public health officers, during the last year.

This is not saying that there has been no progress, that knowledge has remained at a standstill on the matter. Indeed there has been progress steady and sure all the time. Much especially has been learned about the way infections spread and nothing has a more important bearing on disinfection than this.

As to gaseous or room disinfection, formaldehyde still holds the first place in efficiency. There has been, it is true, a return to a certain extent to the use of sulfurous acid, owing to its stronger action upon insect pests, but all the new experiments show clearly that it is a far weaker disinfectant than formaldehyde and cannot be depended upon except in a few limited cases.

Gaseous disinfection as the great panacea for preventing the spread of contagion has been considerably discredited by recent experiments, but the committee are strongly of the opinion that, when properly carried out, it is an important and effective aid in public health work, and will continue to be, and that formaldehyde is by far the best agent to use in carrying it out.

But in using formaldehyde two things must be provided for; (1) a sufficiency of water in the air of the space to be disinfected; and (2) a very rapid disingagement of the gas. Most forms of apparatus fail in one or the other of these points. The experiments of two members of the committee, Dr. H. W. Hill and the chairman, leave no doubt in the matter. They clearly show that not only does efficiency depend upon them, but by properly observing them much time and material can be saved. The disinfection of the future must be simple, short and sharp.

Dr. Hill's work bearing on the distribution of contagious matter in a room, is most instructive and important. The results he found are what some have claimed before, but his experiments are the first to actually establish them for a great many cases. Such experiments should be continued. Among other things they show clearly how an inefficient method of disinfection may apparently give the best of results. If there be no infection in a given room one way of disinfecting it is as good as another. As said before, such results do not show that gaseous disinfection is of no use, but they do show conclusively how great a roll cleanliness and care play in the spread of contagion.

THE DISTRIBUTION OF B. DIPHTHERIÆ AND B. TUBERCULOSIS IN ROOMS OCCUPIED BY PATIENTS SUFFERING FROM THESE DISEASES.

By HIBBERT WINSLOW HILL, M. D.

Director: Boston Board of Health Bacteriological Laboratory.

Last year the committee on disinfection invited information, statistical or experimental, bearing on the necessity or otherwise for the disinfection of rooms occupied by persons having the ordinary infectious diseases.

In pursuance of this suggestion, the writer examined a number of rooms occupied by patients having diphtheria and tuberculosis. Experiments were also made on the dying out by drying of diphtheria bacilli and of tubercle bacilli under ordinary room conditions.

Method of collecting specimens. Ordinary swabs moistened with sterile water were rubbed repeatedly over the suspected areas. Care was taken by the collector, J. F. Broderick, one of the laboratory force, to select exposed surfaces on sheets, pillows, furniture, walls and floor near the patient. At least two swabs, usually more, were rubbed on each area selected. About fifty swabs were taken in each room.

From the diphtheria rooms, each swab was inoculated on a corresponding serum for examination after fifteen hours' incubation. The serum tubes were then arranged in groups, representing respectively, the bed and bed clothing, the floor and furniture, and the walls, or some similar grouping (see tables). The growths from each group were mixed in a corresponding tube of broth, and the mixtures, with or without further incubation, were injected into guinea pigs. Previous experience had shown that apparently-negative mixed cultures from suspected diphtheria patients sometimes yielded diphtheria bacilli when injected into guinea pigs.

The swabs from the tuberculosis rooms were taken in the same manner. Direct microscopic examination of these was found unsatisfactory. The swabs were grouped for inoculation as described above for the diphtheria cultures, each group was shaken with distilled water and the resulting suspension injected into guinea pigs.

Results from diphtheria rooms. These results, given in detail in the tables, may be summarized thus:

Private rooms: Seven rooms were examined, using 335 swabs; the cases were mild, the severer cases in Boston being promptly removed to the hospital; the examinations were made immediately after official release (granted on the obtaining of two consecutive negative cultures from both nose and throat), but of course before disinfection. In every case, the patient whose room was selected had shown at least two good positive cultures during the attack. The findings were as follows:

Room 3; 2 swabs (duplicates) positive from a toy flag; 2 swabs (duplicates) positive from a handkerchief.

Room 7; 11 swabs doubtful from sheet, pillow, mattress, bedframe and floor. All the remaining 320 swabs yielded negative results.

Isolation of each suspicious organism was attempted, but on account of liquefiers, succeeded only in the case of the handkerchief. With this organism, 6 guinea pigs were inoculated under various conditions, but without detecting virulence. The guinea pigs inoculated with the mixed cultures from the swabs all yielded negative results.

Hospital; 3 wards, all occupied at the time of examination, were examined, using 197 swabs; the first ward contained laryngeal cases, all intubed; the second contained mixed cases in various stages; the third contained convalescents still carrying the bacilli. The microscopic results showed Wesbrook's D_2 type on 3 swabs, one each from walls, bedspreads and bedframes in the convalescent ward. The remaining swabs and the guinea pig inoculations were all negative.

Thus, a total of 532 swabs yielded two positive results (in duplicate), and 14 doubtful results thus distributed:

Type A: Four swabs; two each from handkerchief and toy flag of private room 3.

Type A (?); two swabs; one each from sheet and pillow of private room 7.

Type A_1; one swab; from floor of private room 7.

Type C_1; two swabs; one each from bedframe and mattress of the same.

Types C_1 D_2; one swab from mattress of same.

Type D_1; one swab from pillow of same.

Type D_2; seven swabs; one each from wall, bedspread and bedframe in hospital wards; one each from floor, bedframe, sheet and mattress of private room 7.

(Private room 7 was occupied by a child four weeks old which had a very mild infection lasting 4 weeks.)

Results from tuberculosis rooms. These are given in detail in the table, Summarized they are as follows:

Private rooms. Ten rooms were examined, using 496 swabs. The cases were well marked. In 4 cases, the patient had just died, the examinations being made after removal of the body, but before disinfection. The remaining rooms were occupied by the patients. The patients' sputum was positive in 8 cases; in the other two cases it was not examined.

Three rooms yielded positive results as follows:

Room 4; one positive from mixture of swabs from bedframe, floor, wall and furniture; one positive from clothing of patient.

Room 6; one positive from mixture of wall and floor.

Room 8; one positive (in duplicate) from sheet, coverlet and bed; one positive from wall, floor and furniture.

Hospital results. Eighteen rooms were examined, using 180 swabs. Each room was occupied by a single advanced case of tuberculosis. The sputum of the patients was not examined. The swabs were grouped into sets, representing about four rooms each, and each set was subdivided to represent the bed and floor, the walls, and the furniture, doors and windows. All the swabs in each such subdivision were inoculated into one guinea pig.

Two sets of rooms yielded positive results thus:

First set (rooms No. 1, 3, 15, 17), one positive from bed and floor; one positive from furniture, door and window.

Fourth set (rooms No. 31, 33, 35, 36), one positive from floor, door and window.

SOURCES OF ERROR IN THE DIPHTHERIA WORK.

1. *Method of taking swabs:* A table is given showing the results of the control tests intended to determine whether the swab method used on cotton and on wood is thoroughly reliable. It will be seen that, roughly speaking, about one-half the swabs used in these tests yielded positive results. Omitting the eighth day results, which were negative on both cotton and wood, four attempts on cotton yielded three successful results, some positives being obtained in each of three attempts. Four attempts on wood all yielded some successful results. The bacilli had been sprayed upon the test objects from suspensions in water of agar cultures and were doubtless much more abundant in this experiment per square inch of surface than the bacilli to be found in an infected room, but in the tests from infected rooms, duplicate swabs were always taken and often many more, from each suspected area. It is probable then that the swab method as followed in these experiments is fairly reliable.

2. *Cultivation:* In collecting the swabs, many bacteria other than *B. diphtheriae* were necessarily collected also and amongst these, a serum-liquefying bacillus was especially prominent. Fortunately this organism did not grow rapidly enough to spoil the 15-hour-old-cultures, but later on, the cultures were generally liquefied. It is con-

ceivable that *B. diphtheriae*, still living, but weakened by drying, might have been present and yet have failed to develop in time for the 15-hour examination to discover them.

In examining mixed cultures from throats, where the diphtheria bacilli are present and living, the bacilli are missed in about 10 per cent. of cases if but one culture is taken. This error was probably offset in these room tests by the large number of duplicate, triplicate and multiplicate cultures made.

SOURCES OF ERROR IN THE TUBERCULOSIS WORK.

The method of collection of swabs was identical with that used in the diphtheria work and subject to the same errors. The injection of the guinea pigs with the mixture of bacteria from the swabs resulted frequently in the death of the animal before tubercular lesions could have developed; such results are recorded as unsatisfactory. Again, some animals died one month after inoculation without tubercular lesions; these are recorded as questionable negatives. Others, surviving five weeks or more are recorded as positive or negative according to the autopsy findings. It is to be regretted that a larger number of duplicate inoculations were not made.

DRYING EXPERIMENTS.

From diphtheria cultures on agar, 24 hours old at 37 degrees C., glass rods were inoculated in sets, one rod in each set from each culture and each culture coming from a different source. Several sets of rods were exposed in a drying box, arranged to admit diffused daylight and to permit slow diffusion of air. The box stood in the laboratory, subject to ordinary room fluctuations of temperature and humidity. At the end of the various periods given in the tables, sets of rods were removed and inoculated on agar. The tables show the rate at which the cultures died out. Considerable variation was found in different experiments, although no relation between resistance and particular culture was observed. At the end of two weeks, in one experiment, 10 per cent. only of the rods yielded living *B. diphtheriae*. In another experiment, 27 per cent. were still living at this time and at the end of three weeks, 10 per cent. still survived.

From tubercular sputum, containing great numbers of tubercle bacilli, glass rods were similarly prepared. The survival of the bacilli was tested at the end of various periods by inoculation into guinea pigs. The shortest period tested was 16 days, giving a negative result. Most of the tests were from 27 days upwards; all were negative. It is intended to repeat these experiments, using shorter periods.

GENERAL EVIDENCE RELATING TO THESE RESULTS.

Accepting the general results reached above as showing that rooms occupied by tubercular patients are more infected with tubercle bacilli than rooms occupied by diphtheria patients are infected with diphtheria bacilli, it is interesting to attempt to trace the concordance of these results with the general characters of the two diseases. The drying experiments quoted show, so far as they go, that *B. diphtheriae* is more resistant to drying than *B. tuberculosis* under the conditions of the test. It is probable, therefore, that the greater ease in finding the tubercle bacillus is due to its distribution in greater numbers about the room, although the probable greater delicacy of the tests for *B. tuberculosis* must be taken into account.

In diphtheria, *B. diphtheriae* is probably more abundant in the nasopharynx and more abundantly distributed therefrom in the early than in the late stages of the disease. Taking four weeks as the average duration of a case from earliest symptoms to release, a very large proportion of the bacilli thrown out during the first week and reported on exposed surfaces will have died from drying before disinfection is done. A large proportion of those thrown out in the second week will also be dead. Even those of the third week will have suffered some weakening. There will thus be comparatively few organisms left on the exposed surfaces which gaseous disinfectants will reach at the end of the fourth week for the disinfectant to kill. These will be found principally on the bed clothing and body clothing of the patient.

In tuberculosis, on the other hand, the bacilli, although on exposed surfaces they die out about as quickly as *B. diphtheriae*, have their numbers more or less constantly recruited by the coughing, talking and spitting of the patient. As the disease progresses, this distribution is likely to increase rather than to diminish. It is to be noted that some of the positive tubercle results were obtained from places other than the beds of the patients.

In conclusion, it would seem that the evidence for the necessity for surface disinfection after diphtheria is very slight, while the evidence for the necessity of disinfection after tuberculosis is considerably stronger. I wish to acknowledge the courtesy of Dr. J. H. McCollom, the head of the South Department, Boston City Hospital, in facilitating the taking of swabs in the wards and the courtesy of the superintendent of the Cullis Home for Consumptives in extending a similar privilege.

TESTS OF EFFICIENCY OF SWABS FOR REMOVING B. DIPHTHERIAE FROM WOODEN SURFACES AND FROM COTTON SURFACES ON WHICH THE BACILLI HAD BEEN DRIED.

The wooden surface was part of an old varnished box. The cotton was new factory cotton. Both were sprayed with the same emulsion of *B. diphtheriae*, made by suspending a 24-hour agar culture in water. Both were set drying in a protected place in diffuse daylight at ordinary room temperatures and humidity. The swabs were moistened in sterile water before use. A different area of each surface was tested each different time tests were made, but all the swabs used in any one test were taken from the same area. On the second and third days, the order of taking the swabs, rubbed different numbers of times, is shown:

RESULTS FROM COTTON.

Area Tested.	Time Dried.	No. of Swabs.	How Taken.	Pos.	Neg.	Con-tam.
Area No. 1	3 hrs.	2	rubbed repeatedly	0	2	0
" " 2	1 day	2	each rubbed 8 times	2	0	0
" " 3	2 days	1st set 2	" " 1 time	2	0	0
" " 3	"	2d set 2	" " 2 times	1	0	1
" " 3	"	3d set 1	" " 4 "	1	0	0
" " 3	"	4th set 1	" " 6 "	1	0	0
" " 4	3 days	1st set 1	" 6 "	1	0	0
" " 4	"	2d set 1	" 4 "	1	0	0
" " 4	"	3d set 2	" 2 "	2	0	0
" " 4	"	4th set 2	" 1 time	2	0	0
" " 5	8 days	6	rubbed once each	0	4	2
		22		13	6	3

RESULTS FROM WOOD.

Area Tested.	Time Dried.	No. of Swabs.	How Taken.	Pos.	Neg.	Con-tam.
Area No. 1	3 hrs.	2	rubbed repeatedly	2	0	0
" " 2	1 day	2	each rubbed 8 times	1	1	0
" " 3	2 days	1st set 2	" " 1 time	0	2	0
" " 3	"	2d set 2	" " 2 times	2	0	0
" " 3	"	3d set 1	" " 4 "	1	0	0
" " 3	"	4th set 1	" " 6 "	1	0	0
" " 4	3 days	1st set 1	" 6 "	0	1	0
		2d set 1	" 4 "	0	1	0
		3d set 2	" 2 "	2	0	0
		4th set 2	" 1 time	2	0	0
" " 5	8 days	6	rubbed once each	0	5	1
		22		11	10	1

From this table it is seen that, omitting the eighth day results, attempts to obtain positive results from the cotton were successful in three out of the first four tests, while some swabs were positive in each of the four tests made from the wood.

RESULTS FROM DIPHTHERIA CULTURES AND TUBERCULAR SPUTUM DRIED ON GLASS RODS AT ORDINARY ROOM TEMPERATURE AND HUMIDITY.

The diphtheria cultures used came from different sources. Each set of 20, 22, etc., rods represents a corresponding number of these cultures, one rod from each culture. Six sputums were used from six different patients, and are numbered accordingly. The sputum from four rods was used for each inoculation.

PURE CULTURES OF B. DIPHTHERIAE.

Number of Rods Exposed.	20	22	20	22	22	22	99*	22
Number of Days Dying.	1	2	7	7	10	14	14	20
Pos. results...	20	22	16	13	12	6	9	2
Neg. results...	0	0	0	3	5	10	77	15
Contaminated.	0	0	4	6	5	3	13	5

THE SAME IN APPROXIMATE PERCENTAGES.

Number of Days Drying.	1	2	7	7	10	14	14	20
Positive.......	100	100	80	59	54	28	9	9
Negative......	0	0	0	14	23	45	77	68
Contaminated.	0	0	20	28	23	14	13	22

TUBERCULAR SPUTUM.

Sputum No.	Time Dried.	Result in Guineapig
1	16 days	—
1	22 “	unsat.
6	27 “	—
5	33 “	—
1	41 “	—
4	43 “	—
1	51 “	—
3	51 “	—
2	67 “	—
1	82 “	—

* Thirty-three different cultures, exposed in triplicate.

PARTICULARS OF CASES OF DIPHTHERIA IN PRIVATE HOUSES, IN WHICH EXAMINATIONS FOR THE DISTRIBUTIONS OF B. DIPHTHERIAE WERE MADE.

All the private rooms were examined immediately after release and before disinfection.

Patient.	Case 1.	Case 2.	Case 3*	Case 4.		Case 5.	Case 6	Case 7
Sex	Male	female	female	female	female	male	female	female
Age	9 yrs.	14 yrs.	3 yrs.	20 yrs.	24 yrs.	36 yrs.	4 yrs.	4 weeks
Type of attack	mild	mild	mild	mild	mild	mild	mild	mild
No. of positive cultures from patient	3	5	2	1	2	2	3	6
Length of case	17 days	42 days	17 days	14 days	14 days	15 days	13 days	28 days

Room.	Case 1.	Case 2.	Case 3*	Case 4.	Case 5.	Case 6.	Case 7.
Cubic contents	1,500 ft.	700 ft.	1,100 ft.	860 ft.	2,200 ft.	800 ft.	800 ft.
Windows	2	1	1	2	2	1	1
Ventilation	good	good	good	good	good	good	good
Light	"	"	poor	poor	"	poor	"
Cleanliness	"	"	good	good	"	good	"

PARTICULARS OF CASES OF DIPHTHERIA IN THE DIPHTHERIA WARDS OF A HOSPITAL (SOUTH DEPARTMENT BOSTON CITY HOSPITAL).

Similar examinations were made before release of the patients.

Laryngeal ward: Containing seven cases, all intubed, and constantly coughing.

Mixed ward: Containing ten cases in various stages.

Convalescent ward: Containing about 15 patients, still carrying the bacilli.

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* The only undoubted positive results obtained were from this room.

RESULTS OF TESTS MADE IN PRIVATE ROOMS AND IN DIPHTHERIA WARDS OF HOSPITAL TO DETERMINE THE DISTRIBUTION OF B. DIPHTHERIAE ABOUT ROOMS OCCUPIED BY DIPHTHERIA PATIENTS.

	Private Rooms—Showing Swabs Taken.								Hospital Wards Showing Swabs Taken.				Diphtheria-like organisms obtained.			Summary.		
	Case 1.	Case 2.	Case 3.	Case 4.	Case 5.	Case 6.	Case 7.	Total Swabs.	Laryngeal Ward.	Mixed Ward.	Convalescent Ward.	Total Swabs.	Case 3.	Case 7.	Convalescent Ward.	Total Swabs.	Total Negative.	Total Suspicious
Pillows	4	4	4	6	8	8	6	40	11	8	20	39	0	2	0	79	77	2
Sheets	10	8	4	6	10	8	6	52	0	0	0	0	0	2	0	52	50	2
Blankets	0	2	0	2	0	8	0	12	0	0	0	0	0	0	0	12	12	0
Coverlets	4	0	2	0	4	4	0	14	10	0	20	30	0	0	1	44	43	1
Bed frames	6	8	6	6	8	2	18	54	12	2	20	34	0	5	1	88	84	6
Total Swabs	24	22	16	20	30	30	30	172	33	10	60	103	0	9	2	275	266	9
Floor	10	10	2	8	6	3	6	45	10	4	20	34	0	2	0	79	77	2
Wall	6	14	14	10	12	4	14	74	22	4	34	60	0	0	1	134	133	1
Furniture	6	4	2	2	2	2	0	18	0	0	0	0	0	0	0	18	18	0
Total Swabs.	22	28	18	20	20	9	20	137	32	8	54	94	0	2	1	231	228	3
Playthings	0	0	6	0	0	0	0	6	0	0	0	0	2*	0	0	6	4	2
Clothing	4	0	6	6	0	2	0	18	0	0	0	0	0	0	0	18	18	0
Handkerchief	0	0	2	0	0	0	0	2	0	0	0	0	2*	0	0	2	0	2
	4	0	14	6	0	2	0	26	0	0	0	0	4*	0	0	26	22	4
Grand total	50	50	48	46	50	41	50	335	65	18	114	197	4*	11	3	532	516	16

* These were undoubtedly true diphtheria bacilli. It remains a question whether or not the other suspicious organisms were true B. diphtheriæ or not.

PARTICULARS OF CASES OF PULMONARY TUBERCULOSIS IN PRIVATE HOUSES IN WHICH THE ROOMS OCCUPIED BY THE PATIENT WERE EXAMINED FOR THE PRESENCE OF B. TUBERCULOSIS.

In four cases the rooms were examined immediately after the death of the patient, and before the disinfection. In the remaining cases, the rooms were still occupied by the patients at the time of examination:

Patient	Case 1	Case 2	Case 3	Case 4	Case 5	Case 6	Case 7	Case 8	Case 9	Case 10
Condition at time of test.	dead	dead	dead	dead	living	living	living	living	living	living
Sex	female	male	male	male	male	female	male	female	female	male
Age	adult	adult	30 yrs.	40 yrs.	24 yrs.	adult	30 yrs.	21 yrs.	30 yrs.	27 yrs.
Type of case..	severe	severe	severe	severe	severe	mild	severe	severe	mild	mild
Length of case	years	mo'ths	4 mos.	6 mos.	8 mos.	7 mos.	mo'ths	3 mos.	2 mos.	1 mo.
Patient's sputum	not exam.	not exam.	+	+	+	+	+	+	+	+
Room										
Cubic contents	700 ft.	800 ft.	800 ft.	1,200 ft.	1,300 ft.	1,200 ft.	600 ft.	1,200 ft.	700 ft.	700 ft.
Windows	1	1	1	2	2	2	1	2	1	1
Doors	1	1	1	2	1	1	1	1	2	2
Ventilation ...	good	poor	fair	good	good	good	poor	good	good	good
Light	good	poor	poor	good	good	good	bad	fair	good	good
Cleanliness ...	fair	fair	bad	good	good	good	bad	bad	good	good
Spittoon	no	no	no	no	no	no	no	no	no	no

RESULTS OF TESTS MADE FROM PRIVATE ROOMS OCCUPIED BY TUBERCULAR PATIENTS FOR THE PRESENCE OF B. TUBERCULOSIS IN AND ABOUT THE ROOMS.

The guinea pigs inoculated from the swabs of any one room were lettered from A on, for identification. The table indicates what swabs were inoculated into each pig in each case:

Swabs taken from	Case 1		Case 2		Case 3		Case 4		Case 5		Case 6		Case 7		Case 8		Case 9		Case 10		Total Swabs
Pillow	6	A	6	A	16	A	8	A	8	A	8	A	10	A A	0		8	A A	8	A A	
Sheet	10	A	8	B	0		0		16	B	8	B	6	A A	10	A A	8	A A	8	A A	
Coverlet ..	4	A	6	C	0		6	B	0		0		4	A A	4	A A	8	A A	4	A A	
													4	A A							
Bedframe	2	A	4	D	0		8	C	4	B	8	B	4	B B	8	A A	6	A A	10	A A	
Total...	22		24		16		22		28		24		28		22		30		30		246
Door	4	B	4	E	6	B	2	C	6	C	4	C	6	B B	6	B B	4	B B	6	B B	
Wall	6	C	8	F	16	B	10	C	8	D	8	C	10	B B	16	B B	10	B B	8	B B	
Furniture.	10	D	6	G	12	B	2	C	4	C	6	D	2	B B	6	B B	4	B B	6	B B	
Total...	20		18		34		14		18		18		18		28		18		20		206
Clothing...	6	A	8	H	0		14	D	4	E	8	E	4	A A	0		4		0		44
Grand Total...	48		50		50		50		50		50		50		50		48		50		496

RESULTS OF GUINEA PIG INOCULATIONS.

	Case 1		Case 2		Case 3		Case 4		Case 5		Case 6		Case 7		Case 8		Case 9		Case 10		
	A	—?	A	—?	A	—	A	—	A	Unsat.	A	—	A	—	A	+	A	—	A	—	
	B	—?	E	—	B	—	B	—	B	—	B	Unsat.	A	Unsat.	A	+	A	Unsat.	A	—	
	C	Unsat.	H	—?			C	+	C	—	C	+	B	Unsat.	B	+	B	—	B	—	
	D	—	B C D F G	Unsat.			D	+	D	—?	D	Unsat.	B	Unsat.	B	Unsat.	B	Unsat.	B	—	
									E	—?	E	Unsat.									

Autopsies were made ultimately in all cases surviving inoculation more than a few days.

The signs used are: Unsat = death without tubercular lesions within three weeks of inoculation; —? = death without tubercular lesions within four weeks of inoculation; — = survival for five weeks without tubercular lesions.

RESULTS OF TESTS MADE FROM ROOMS IN A HOSPITAL FOR CONSUMPTIVES FOR THE PRESENCE OF B. TUBERCULOSIS IN AND ABOUT THE ROOMS.

Eighteen rooms were tested in all. Each was occupied by one patient, having advanced pulmonary tuberculosis. The patients included both sexes and were, with one exception, adults. Spittoons were used. The rooms were not large, but were clean, well ventilated and well lighted. The swabs from the rooms were grouped in sets representing four rooms each, each set being subdivided as to source, as in the similar work in private rooms, and injected into guinea pigs.

	Rooms 1-3-15-17			Rooms 19-23-24-25			Rooms 26-28-29-30			Rooms 31-33-35-36			Rooms 38-40			
	No. of Swabs	Guinea Pigs Inoc.	Results	No. of Swabs	Guinea Pigs	Results	Swabs	Guinea Pigs	Results	Swabs	Guinea Pigs	Results	Swabs	Guinea Pigs	Results	Total Swabs
Bed and floor....	16	A	+	16	A	—	16	A	—	16	A	—	8	A	—	72
Wall............	8	B	—?	8	B	—?	8	B	—?	8	B	—	4	B	—	36
Furniture, door, window........	16	C	+	16	C	—	16	C	—?	16	C	+	8	C	—	72
	40			40			40			40			20			180

Autopsies were made ultimately on all guinea pigs surviving inoculation three weeks or more. Those marked —? died about one month after inoculation without showing tubercular lesions.

EXPERIMENTS IN DISINFECTION WITH FORMALDEHYD GAS.

By MAZYCK P. RAVENEL, M. D., and
S. H. GILLILAND, V. M. D.

(From the Laboratory of the State Live Stock Sanitary Board of Pennsylvania.)

It is universally accepted that formaldehyd gas is one of the most useful and efficient germicides that we possess, but it is equally clear that the ideal method of application for the disinfection of dwellings, stables, etc., has not yet been demonstrated. The difference of opinion among those who have experimented with formaldehyd gas may be ascribed in part, at least, to the effect of certain factors, as temperature and moisture, which are as yet imperfectly understood.

The experiments which form the subject of this paper were done on a practical basis in rooms and buildings not especially prepared for such experiments. They represent, then, conditions met with in actual work, and results which may be obtained in such work.

Four methods were employed:

1. Generation of the gas from methyl alcohol. In the generator employed the vapor of the alcohol is passed between two cones of platinized asbestos, and also through five layers of 20 mesh copper wire, only sufficient air being admitted by a series of small holes for the dehydrogenation of the vapor. A seal of water prevents access of air directly to the reservoir of alcohol, and at the same time supplies moisture for the hydration of the formaldehyd gas, thereby increasing its germicidal power. The generator must be left in the room which is being disinfected, there being no method of collecting the gas and passing it into a room from the outside.

2. Evolution of the gas from the commercial 40 per cent. solution by means of a retort without pressure. The apparatus employed consisted of a copper retort which has a long neck. The interior of the retort is divided into compartments, under each of which a burner is so placed that the heat is applied at the periphery. Riveted to the bottom is a circular plate of asbestos, which prevents the heat from reaching the central portion of the retort, thereby keeping a column of liquid above it comparatively cool. In this manner the liquid is made to circulate by following up the periphery of the retort and descending through the center. The entire retort is jacketed, so that

the long neck is kept at a high temperature, contact with which facilitates the disengagement of the gas. The construction is especially designed to prevent priming and ejection of the liquid into the room, as well as to drive off the gas rapidly, both of which are successfully accomplished.

This apparatus may be used from the outside by passing the nozzle through the keyhole, or else be left in the room to burn until the alcohol in the lamp which furnishes the heat is exhausted. When this method is employed the amount of alcohol placed in the lamp must be so proportioned that it will completely evaporate the solution in the retort, the general amount necessary being one ounce of alcohol for every two ounces of solution.

3. Evolution of the gas from the commercial 40 per cent. solution by evaporation from large surfaces.

4. A combination of methods 2 and 3.

OUTLINE OF THE GENERAL METHOD USED THROUGHOUT THE FOLLOWING EXPERIMENTS.

Cultures. Staphylococcus pyogenes aureus, typhoid, diphtheria, tuberculous sputum, anthrax, colon, hog cholera, spore anthrax, glanders and tubercle bacilli.

Method. Suspensions of the above cultures were either made in sterile water or bouillon until the opacity was equal to a 24-hour-old bouillon culture of typhoid. Strips of sterile filter paper, one and one-half inches long by three-eighths of an inch wide, were dipped in these suspensions and placed between single layers of sterile muslin. These strips were prepared for each experiment just prior to the time of exposure.

The pieces of muslin were then distributed around the room at various points and under different conditions as will be described separately with each experiment. After these had been subjected to the action of the gas for a given length of time, they were transferred to tubes of bouillon and observed for 72 hours.

Control cultures were used in a large number of the experiments as indicated.

In all cases where there was growth at the end of 72 hours the same was identified.

In the tabulation of the results the — sign indicates no growth or killed, while the + sign indicates growth or not killed.

The mean temperature and humidity for the day of exposure is the one recorded.

EXPERIMENT I.

MARCH 25, 1901.

Machine—Type I. Gas generated from methyl alcohol.

Weather—Temperature, 50½ F. Humidity, 89½.

Cultures—Anthrax free from spores, staphylococcus-aureus, typhoid, diphtheria and hog cholera.

Room—Brick walls containing five windows, one door, one ventilator and one drain. Capacity, 1800 cubic feet.

Methyl alcohol, 1800 c. c., used.

DISTRIBUTION OF CULTURES.

No. 1. Between layers of horse-blanket on floor—two layers on top and six layers below.

No. 2. Between layers of horse-blanket on floor—four layers on top and four layers below.

No. 3. On platform six feet high, in closely woven blanket—two layers on top and six layers below. Platform not solid.

No. 4. On platform—four layers of blanket on top and four layers below.

No. 5. On surface of blanket on floor.

No. 6. On surface of blanket on platform.

No. 7. One culture of each was sealed in an ordinary white envelope, three of these put on floor and three on platform.

Room remained closed 17 hours.

RESULTS.

	Anthrax.		
	24 hrs.	48 hrs.	72 hrs.
1	+	+	+
2	+	+	+
3	+	+	+
4	+	+	+
5	Lost		
6	+	+	+
7	—	—	—

	Typhoid.		
	24 hrs.	48 hrs.	72 hrs.
1	—	+	+
2	+	+	+
3	—	+	+
4	—	+	+
5	—	—	—
6	—	—	—
7	—	—	—

	Hog Cholera.		
	24 hrs.	48 hrs.	72 hrs.
1	—	+	+
2	+	+	+
3	—	—	—
4	+	+	+
5	—	—	—
6	—	—	—
7	—	—	—

	Aureus.		
	24 hrs.	48 hrs.	72 hrs.
1	+	+	+
2	+	+	+
3	—	+	+
4	—	+	+
5	—	—	—
6	—	—	—
7	—	—	—

	Diphtheria.		
	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	+	+
3	—	—	—
4	—	+	+
5	—	—	—
6	—	—	—
7	—	—	—

EXPERIMENT II.

MARCH 26, 1901.

Machine—Type I. Gas generated from methyl alcohol.

Weather—Temperature, 49° F. Humidity, 79½. Rain, 11:25 a. m to 3:30 p. m.

Cultures, room, quantity of alcohol used, and the distribution of cultures the same as in Experiment I.

Room remained closed 17¾ hours.

RESULTS.

Anthrax.

	24 hrs.	48 hrs.	72 hrs.
1	+	+	+
2	+	+	+
3	+	+	+
4	+	+	+
5	—	—	—
6	—	—	—
7	—	—	—

Aureus.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	+	+
5	—	—	—
6	—	—	—
7	—	—	—

Typhoid.

	24 hrs.	48 hrs.	72 hrs.
1	—	+	+
2	—	+	+
3	Lost		
4	—	+	+
5	—	—	—
6	—	—	—
7	—	—	—

Hog Cholera.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	+	+
3	—	—	—
4	—	+	+
5	—	—	—
6	—	—	—
7	—	—	—

Diphtheria.

	24 hrs.	48 hrs.	72 hrs.
1	—	+	+
2	—	+	+
3	—	—	—
4	—	+	+
5	—	—	—
6	—	—	—
7	—	—	—

EXPERIMENT III.

March 28, 1901.

Machine—Type I. Gas generated from methyl alcohol.

Weather—Temperature, 38½° F. Humidity, 59. Rain in early morning.

Cultures—Anthrax, staphylococcus aureus, typhoid, hog cholera, diphtheria, sputum from a tuberculous patient containing various bacteria was exposed under the first four methods of distribution.

Room, quantity of alcohol used, and the distribution of cultures the same as in Experiments I and II.

Room remained closed six hours.

RESULTS.

Anthrax.

	24 hrs.	48 hrs.	72 hrs.
1	+	+	+
2	+	+	+
3	+	+	+
4	+	+	+
5	+	+	+
6	—	—	—
7	+	+	+

Aureus.

	24 hrs.	48 hrs.	72 hrs.
1	—	+	+
2	+	+	+
3	—	+	+
4	+	+	+
5	—	—	—
6	—	—	—
7	—	—	—

Typhoid.

	24 hrs.	48 hrs.	72 hrs.
1	+	+	+
2	+	+	+
3	—	—	+
4	+	+	+
5	—	—	—
6	—	—	—
7	—	—	—

Hog Cholera.

	24 hrs.	48 hrs.	72 hrs.
1	—	+	+
2	+	+	+
3	—	+	+
4	+	+	+
5	—	—	—
6	—	—	—
7	—	—	—

Diphtheria.

	24 hrs.	48 hrs.	72 hrs.
1	—	+	+
2	—	—	+
3	—	—	+
4	—	+	+
5	—	—	—
6	—	—	—
7	—	—	—

Sputum.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	+	+	+
3	—	—	—
4	+	+	+

EXPERIMENT IV.

April 9, 1901.

Machines—Type I. Gas generated from methyl alcohol.

Weather—Temperature, 45° F. Humidity, 62. Cloudy. Wind high all night.

Cultures—Anthrax, staphylococcus aureus, diphtheria, hog cholera, typhoid and glanders.

Room—Lecture room with eight large windows and one double door. All cracks sealed with manilla paper. Capacity, 5600 cubic feet.

Methyl alcohol—4500 c. c. used, divided equally among three machines.

DISTRIBUTION OF CULTURES.

No. 1. On desks in middle of room between two layers of muslin loosely folded.

No. 2. On side desks.

No. 3. In drawers of desks.

No. 4. On top of microscope case.

No. 5. Between two layers of woolen blanket hung on chandelier in middle of room about five feet from floor.

No. 6. On floor in middle of room.

No. 7. In sealed envelopes on desks in middle of room.

No. 8. In pockets of coat hung on chandelier.

Room remained closed 18 hours.

RESULTS.

	Anthrax.				Aureus.				Diphtheria.		
	24 hrs.	48 hrs.	72 hrs.		24 hrs.	48 hrs.	72 hrs.		24 hrs.	48 hrs.	72 hrs.
1	—	—	—	1	—	—	—	1	—	—	—
2	—	—	—	2	—	—	—	2	—	—	—
3	—	+	+	3	—	—	—	3	—	—	—
4	—	—	—	4	—	—	—	4	—	—	—
5	—	—	—	5	—	—	—	5	—	—	—
6	—	—	—	6	—	—	—	6	—	—	—
7	—	—	—	7	—	—	—	7	—	—	—
				8	—	—	—	8	—	+	+

	Glanders.				Typhoid.				Hog Cholera.		
	24 hrs.	48 hrs.	72 hrs.		24 hrs.	48 hrs.	72 hrs.		24 hrs.	48 hrs.	72 hrs.
1	—	—	—	1	—	—	—	1	—	—	—
2	—	—	—	2	+	+	+	2	—	—	—
3	—	—	—	3	—	—	—	3	—	—	+
4	—	—	—	4	—	—	—	4	—	—	—
5	—	—	—	5	—	—	—	5	—	—	—
6	—	—	—	6	—	—	—	6	—	—	—
7	—	—	—	7	—	—	—	7	—	—	—
				8	—	—	—	8	—	—	—

EXPERIMENT V.

April 10, 1901.

Machines—Type I. Gas generated from methyl alcohol.

Weather—Temperature, 46½° F. Humidity, 59½. Cloudy.

Cultures—Anthrax, staphylococcus aureus, diphtheria, typhoid, hog cholera and glanders.

Room—Cement floor with enameled walls, six windows, one drain, one double door, and three ventilators. All cracks sealed with manilla paper. Capacity, 3568 cubic feet.

Methyl alcohol—2750 c. c. used, divided equally among two machines.

DISTRIBUTION OF CULTURES.

No. 1. Directly exposed on floor.

No. 2. On floor in sealed envelopes.

No. 3. Directly exposed on cage four feet high.

No. 4. On cage between four layers of woolen blanket.
No. 5. On cage between two layers of blanket.
No. 6. In opened envelopes in coat pocket.
No. 7. On ventilator 8 feet high.
Room remained closed 17¾ hours.

RESULTS.

	Anthrax.				Aureus.				Diphtheria.		
	24 hrs.	48 hrs.	72 hrs.		24 hrs.	48 hrs.	72 hrs.		24 hrs.	48 hrs.	72 hrs.
1	—	—	—	1	—	—	—	1	—	—	—
2	—	—	—	2	—	—	—	2	—	—	—
3	—	—	—	3	—	—	—	3	—	—	—
4	—	—	—	4	—	—	—	4	—	—	—
5	—	—	—	5	—	—	—	5	—	—	—
6	—	—	—	6	—	—	—	6	—	—	—
7	—	—	—	7	—	—	—	7	—	—	—

	Typhoid.				Hog Cholera.				Glanders.		
	24 hrs.	48 hrs.	72 hrs.		24 hrs.	48 hrs.	72 hrs.		24 hrs.	48 hrs.	72 hrs.
1	—	—	—	1	—	—	—	1	—	—	—
2	—	—	—	2	—	—	—	2	—	—	—
3	—	—	—	3	—	—	—	3	—	—	—
4	—	—	—	4	—	—	—	4	—	—	—
5	—	—	—	5	—	—	—	5	—	—	—
6	—	—	—	6	—	—	—	6	—	—	—
7	—	—	—	7	—	—	—	7	—	—	—

EXPERIMENT VI.

April 12, 1901.

Machines—Type I. Gas generated from methyl alcohol.

Weather—Temperature, 50½° F. Humidity, 49½. Clear.

Cultures—Anthrax, diphtheria, glanders, staphylococcus aureus, typhoid and hog cholera.

Room—Stable in brick building, ground floor, three windows and one door. Capacity, 3340 cubic feet.

Methyl alcohol—2500 c. c. used, divided equally between two machines.

DISTRIBUTION OF CULTURES.

No. 1. In sealed envelopes on floor.
No. 2. Under four inches of straw on floor.
No. 3. On stanchion rail four feet high.
No. 4. In opened envelopes in pockets of coat.
No. 5. In loft nine feet high.
Room remained closed 18½ hours.

RESULTS.

	Aureus.				Anthrax.				Typhoid.		
	24 hrs.	48 hrs.	72 hrs.		24 hrs.	48 hrs.	72 hrs.		24 hrs.	48 hrs.	72 hrs.
1	—	+	+	1	—	—	—	1	—	—	—
2	+	+	+	2	—	—	—	2	+	+	+
3	—	—	—	3	—	—	—	3	—	—	—
4	—	—	—	4	—	—	—	4	—	—	—
5	—	—	—	5	—	—	—	5	—	—	—

Hog Cholera.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	+	+	+
3	—	—	—
4	—	—	—
5	—	—	—

Diphtheria.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—

Glanders.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	+	+
3	—	—	—
4	—	—	—
5	—	—	—

EXPERIMENT VII.

APRIL 15, 1901.

Machines—Same as Experiment VI.

Weather—Temperature, 44½° F. Humidity, 76½. Rain at intervals.

Cultures—Anthrax, staphylococcus aureus, colon, typhoid, diphtheria and glanders.

Room, quantity of alcohol used and the distribution of cultures the same as in Experiment VI. Room remained closed 20 hours.

RESULTS.

Anthrax.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—

Aureus.

	24 hrs.	48 hrs.	72 hrs.
1	+	+	+
2	+	+	+
3	+	+	+
4	—	—	+
5	—	+	+

Typhoid.

	24 hrs.	48 hrs.	72 hrs.
1	—	+	+
2	+	+	+
3	—	+	+
4	—	—	—
5	—	—	—

Colon.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	+	+	+
3	—	—	—
4	—	—	—
5	—	—	—

Diphtheria.

	24 hrs.	48 hrs.	72 hrs.
1	+	+	+
2	+	+	+
3	—	—	+
4	—	—	+
5	—	—	—

Glanders.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	+	+	+
4	—	—	—
5	—	—	—

EXPERIMENT VIII.

APRIL 17, 1901.

Machine—Type I. Gas generated from methyl alcohol.

Weather—Temperature 53° F. Humidity, 51. Clear.

Cultures—Anthrax, spore anthrax, staphylococcus aureus, glanders, typhoid, colon, diphtheria and Asiatic cholera.

Room—Brick room in experimental building, walls painted black, one window, one ventilator and one door. Capacity, 1400 cubic feet.

Methyl alcohol—1500 c. c. used.

DISTRIBUTION OF CULTURES.

No. 1. In sealed envelopes on floor.
No. 2. On floor between two layers of muslin.
No. 3. On barrel 3½ feet high between four layers of blanket.
No. 4. In envelope box between two layers of muslin.
No. 5. In open envelope in pocket of coat.
Room remained closed 18¾ hours.

RESULTS.

Anthrax.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—

Spore Anthrax.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—

Aureus.

	24 hrs.	48 hrs.	72 hrs.
1	—	+	+
2	—	—	—
3	—	+	+
4	—	+	+
5	—	—	—

Asiatic Cholera.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—

Colon.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—

Typhoid.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	+	+
5	+	+	+

Diphtheria.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	+	+
5	—	—	—

Glanders.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—

EXPERIMENT IX.

April 23, 1901.

Machine—Type I. Gas generated from methyl alcohol.

Weather—Temperature, 52° F. Humidity, 79. Cloudy.

Cultures—Anthrax, staphylococcus aureus, colon, typhoid, Asiatic cholera, glanders and diphtheria.

Room, quantity of alcohol used, and distribution of cultures the same as Experiment VIII. Room remained closed for six hours.

RESULTS.

Anthrax.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—

Aureus.

	24 hrs.	48 hrs.	72 hrs.
1	—	+	+
2	—	—	—
3	—	+	+
4	—	+	+
5	—	+	+

Colon.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—

Typhoid.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—

Cholera.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—

Glanders.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—

Diphtheria.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—

EXPERIMENT X.

APRIL 25, 1901.

Machines—Type I. Gas generated from methyl alcohol.

Weather—Temperature, 54° F. Humidity, 84½. Rain.

Cultures—Anthrax, spore anthrax, staphylococcus aureus, colon, typhoid, Asiatic cholera, glanders and diphtheria.

Room—Cement floor, enameled walls, containing four windows, two ventilators, one drain and one double door. Capacity, 3058 cubic feet.

Methyl alcohol—2500 c. c. used, equally divided between two machines.

DISTRIBUTION OF CULTURES.

No. 1. In opened envelopes on floor.

No. 2. Directly exposed on floor.

No. 3. Between layers of woolen blanket on wire cage four feet high—two layers on top and six layers below.

No. 4. Same as No. 3, with four layers on top and four layers below.

No. 5. In pocket of coat.

Room remained closed for 18½ hours.

RESULTS.

Anthrax.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—

Spore Anthrax.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—

Aureus.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—

Typhoid.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—

Colon.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—

Typhoid.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—

Glanders.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—

Diphtheria.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—

EXPERIMENT XI.

APRIL 27, 1901.

Machine—Type I. Gas generated from methyl alcohol.

Weather—Temperature, 54° F. Humidity, 29½. Clear.

Cultures—Anthrax, spore anthrax, staphylococcus aureus, colon, typhoid, Asiatic cholera, glanders and diphtheria.

Room—Brick, with wall painted black, cement floor, one window, one ventilator and one door. Capacity, 1400 cubic feet.

Methyl alcohol—1500 c. c. used.

DISTRIBUTION OF CULTURES.

No. 1. In opened envelope on floor.

No. 2. Directly exposed on floor.

No. 3. Between layers of woolen blanket—two layers on top with six layers below.

No. 4. Same as No. 3, with four layers on top and four layers below.

No. 5. Directly exposed on elevation six feet high.

Room remained closed six hours.

RESULTS.

	Anthrax.				Spore Anthrax.				Aureus.		
	24 hrs.	48 hrs.	72 hrs.		24 hrs.	48 hrs.	72 hrs.		24 hrs.	48 hrs.	72 hrs.
1	—	—	—	1	—	—	—	1	—	—	—
2	—	—	—	2	—	—	—	2	—	—	—
3	—	—	—	3	—	—	—	3	—	—	—
4	—	—	—	4	—	—	—	4	—	—	+
5	—	—	—	5	—	—	—	5	—	—	—

	Typhoid.				Colon.				Cholera.		
	24 hrs.	48 hrs.	72 hrs.		24 hrs.	48 hrs.	72 hrs.		24 hrs.	48 hrs.	72 hrs.
1	—	—	—	1	—	—	—	1	—	—	—
2	—	—	—	2	—	—	—	2	—	—	—
3	—	—	—	3	—	—	—	3	—	—	—
4	—	—	—	4	—	—	—	4	—	—	—
5	—	—	—	5	—	—	—	5	—	—	—

	Diphtheria.				Glanders.		
	24 hrs.	48 hrs.	72 hrs.		24 hrs.	48 hrs.	72 hrs.
1	—	—	—	1	—	—	—
2	—	—	—	2	—	—	—
3	—	—	—	3	—	—	—
4	—	—	—	4	—	—	—
5	—	—	—	5	—	—	—

EXPERIMENT XII.

June 13, 1901.

Machine—Type II. Gas generated from formaldehyd solution.

Weather—Temperature, 74° F. Humidity, 81½. Partly cloudy.

Cultures—Aureus, typhoid and diphtheria.

Room—In a brick building with a cement floor. Five windows, one door, one drain and one ventilator. Capacity, 1560 cubic feet.

Formaldehyd solution—Sixteen (16) ounces used with 1 per cent. glycerin. (Rate, 10 ounces to 1000 cubic feet.)

DISTRIBUTION OF CULTURES.

Cultures marked 1 were on floor between layers of woolen blanket—two layers on top and six layers below.

Cultures marked 2 were on floor between layers of woolen blanket—four layers on top and four layers below.

Cultures marked 3 were on platform six feet high between layers of woolen blanket—four layers on top and four layers below .

Cultures marked 4 were in sealed envelopes on surface of blanket on floor.

Cultures marked 5 were exposed directly on blanket on platform.

Gas generated from within the room.

Room remained closed for 19 hours.

RESULTS.

	Aureus.				Typhoid.				Diphtheria.		
	24 hrs.	48 hrs.	72 hrs.		24 hrs.	48 hrs.	72 hrs.		24 hrs.	48 hrs.	72 hrs.
1	—	—	—	1	—	—	—	1	—	—	—
2	—	—	—	2	—	—	—	2	—	—	—
3	—	—	—	3	—	—	—	3	—	—	—
4	—	—	—	4	—	—	—	4	—	—	—
5	—	—	—	5	—	—	—	5	—	—	—

EXPERIMENT XIII.

JUNE 15, 1901.

Machine—Type II. Gas generated from formaldehyd solution.

Weather—Temperature, 64° F. Humidity, 81. Rain till 3 p. m.

Cultures—Aureus, typhoid, diphtheria and anthrax.

Room—Same as in Experiment XII.

Formaldehyd solution—Sixteen (16) ounces used with 1 per cent. glycerin. (10 ounces to 1000 cubic feet.)

Alcohol—Eleven (11) ounces were used after wicks were saturated.

DISTRIBUTION OF CULTURES.

Same as in Experiment XII.

Gas generated from the outside of the room through the keyhole.

Room remained closed for 21 hours.

RESULTS.

	Aureus.				Typhoid.		
	24 hrs.	48 hrs.	72 hrs.		24 hrs.	48 hrs.	72 hrs.
1	—	—	—	1	—	—	—
2	—	—	—	2	—	—	—
3	—	—	—	3	—	—	—
4	—	—	—	4	—	—	—
5	—	—	—	5	—	—	—

	Diphtheria.				Anthrax.		
	24 hrs.	48 hrs.	72 hrs.		24 hrs.	48 hrs.	72 hrs.
1	—	—	—	1	—	—	—
2	—	—	—	2	—	—	—
3	—	—	—	3	—	—	—
4	—	—	—	4	—	—	—
5	—	—	—	5	—	—	—

EXPERIMENT XIV.

JUNE 12, 1901.

Machine—Type II. Gas generated from formaldehyd solution.

Weather—Temperature, 66½° F. Humidity, 60. Clear.

Cultures—Aureus, typhoid, diphtheria and anthrax.

Room—Same as in Experiments XII and XIII.

Formaldehyd solution—Sixteen (16) ounces used, with 1 per cent. glycerin. (10 ounces to 1000 cubic feet.)

Alcohol—Eleven (11) ounces used after wicks were saturated.

DISTRIBUTION OF CULTURES.

Same as in Experiments XII and XIII.

Gas generated from within the room.

Room remained closed 11 hours.

RESULTS.

		Aureus.				Typhoid.	
	24 hrs.	48 hrs.	72 hrs.		24 hrs.	48 hrs.	72 hrs.
1	—	—	—	1	—	—	—
2	—	+	+	2	—	—	—
3	—	—	—	3	—	—	—
4	—	—	—	4	—	—	—
5	—	—	—	5	—	—	—

		Diphtheria.				Anthrax.	
	24 hrs.	48 hrs.	72 hrs.		24 hrs.	48 hrs.	72 hrs.
1	—	—	—	1	—	—	—
2	—	—	—	2	—	+	+
3	—	—	—	3	—	—	—
4	—	—	—	4	—	—	—
5	—	—	—	5	—	—	—

REMARKS. From comparison with Experiments XII and XIII, it is probable that the short exposure is the cause of the growth of the aureus and anthrax.

EXPERIMENT XV.

JUNE 19, 1901.

Machines—Type II. Gas generated from formaldehyd solution. Two machines used.

Weather—Temperature, 70½° F. Humidity, 76½. Clear.

Culture—Aureus, typhoid, diphtheria and anthrax.

Room—Lecture room with seven large windows, two doors and one ventilator. All cracks around doors sealed with heavy manilla paper. Capacity, 5600 cubic feet.

Formaldehyd solution—Fifty-six ounces used, with 1 per cent. glycerin. (28 ounces in each machine. 10 ounces to every 1000 cubic feet.)

Alcohol—Thirty-two ounces used after wicks were saturated. (16 ounces in each machine.)

DISTRIBUTION OF CULTURES.

No. 1. In drawers of center and side tables with the drawers closed.

No. 2. On top of centre and side tables.

No. 3. On floor between layers of woolen blanket—two layers on top and six layers underneath.

No. 4. Suspended from chandelier five feet from floor between layers of woolen blanket—four layers on top and four layers underneath.

No. 5. In sealed envelopes on top of microscope case eight feet from floor.

No. 6. Control. Kept under similar conditions in another room.

Gas generated from within the room.

Room remained closed 17½ hours.

RESULTS.

		Aureus.				Typhoid.	
	24 hrs.	48 hrs.	72 hrs.		24 hrs.	48 hrs.	72 hrs.
1	—	—	—	1	—	—	—
2	—	—	—	2	—	—	—
3	—	—	—	3	—	—	—
4	—	—	—	4	—	—	—
5	—	—	—	5	—	—	—
6	+	+	+	6	—	+	+

	Diphtheria.		
	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—
6	+	+	+

	Anthrax.		
	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—
6	+	+	+

EXPERIMENT XVI.

June 26, 1901.

Weather—Temperature, 81° F. Humidity, 67½. Rain.

Cultures, machine, room, quantity of formaldehyd solution and alcohol used, as well as the distribution of cultures, the same as in Experiment XV.

Gas generated from the outside of the room.

One machine used.

Room remained closed for 24 hours.

RESULTS.

	Aureus.		
	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—
6	+	+	+

	Typhoid.		
	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—
6	+	+	+

	Diphtheria.		
	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—
6	+	+	+

	Anthrax.		
	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—
6	+	+	+

EXPERIMENT XVII.

June 26, 1901.

Machine—Type II. Gas generated from formaldehyd solution.

Weather—Temperature, 81° F. Humidity, 67½. Rain.

Cultures—Colon, hog cholera and spore anthrax.

Room—Medium sized bedroom. Two windows and one door. Cracks of the windows and door sealed with heavy manilla paper. Capacity, 1500 cubic feet.

Formaldehyd solution—Eighteen (18) ounces used, with 1 per cent, glycerin. (Rate, 12 ounces to 1000 cubic feet.)

Alcohol—Nine (9) ounces used after wicks were saturated. (Rate, one ounce to two ounces of formaldehyd.)

DISTRIBUTION OF CULTURES.

No. 1. Directly exposed on top of table near window.

No. 2. On top of springs of bed, between layers of woolen blankets—six layers above and six layers underneath.

No. 3. In drawers of table with drawers closed.

No. 4. On floor between layers of woolen blanket—two layers on top and six layers underneath.

No. 5. Control. Kept under similar conditions in another room.

Room remained closed for 23½ hours.

Remarks. While the gas was being generated there was considerable formaldehyd forced out with it—would say about one-third the amount in retort. This was found to be due to the compartments of the retort not being properly placed over burners of lamp.

The gas was generated from the inside of the room.

RESULTS.

	Colon.				Hog Cholera.				Spore Anthrax.		
	24 hrs.	48 hrs.	72 hrs.		24 hrs.	48 hrs.	72 hrs.		24 hrs.	48 hrs.	72 hrs.
1	—	—	—	1	—	—	—	1	—	—	—
2	—	—	—	2	—	—	—	2	+	+	+
3	—	—	—	3	—	—	—	3	+	+	+
4	—	—	—	4	—	—	—	4	—	—	—
5	+	+	+	5	+	+	+	5	+	+	+

EXPERIMENT XVIII.

June 28, 1901.

Weather—Temperature, 81½° F. Humidity, 72. Partly cloudy.

Cultures—Colon, hog cholera and spore anthrax.

Room, machine, quantity of formaldehyd solution used, and the distribution of cultures the same as in previous Experiment No. XVII.

Gas generated from the interior of the room.

Room remained closed for 24 hours.

RESULTS.

	Colon.				Hog Cholera.				Spore Anthrax.		
	24 hrs.	48 hrs.	72 hrs.		24 hrs.	48 hrs.	72 hrs.		24 hrs.	48 hrs.	72 hrs.
1	—	—	—	1	—	—	—	1	—	—	—
2	—	—	—	2	—	—	—	2	+	+	+
3	—	—	—	3	—	—	—	3	+	+	+
4	—	—	—	4	—	—	—	4	—	—	—
5	+	+	+	5	+	+	+	5	+	+	+

EXPERIMENT XIX.

June 28, 1901.

Machine—Type II. Gas generated from formaldehyd solution.

Weather—Temperature, 81½° F. Humidity, 72. Partly cloudy.

Cultures—Aureus, typhoid, diphtheria and anthrax.

Room—In cellar containing four windows, one drain and one door. Capacity, 4200 cubic feet. There were many cracks which could not be sealed shut. It also contained many rat holes and both the windows and door fit loosely.

Formaldehyd solution—Forty-two ounces used, with 1 per cent. glycerin. (10 ounces to every 1000 cubic feet.)

DISTRIBUTION OF CULTURES.

No. 1. On floor between layers of woolen blanket—four layers on top and four layers underneath.

No. 2. Directly exposed on top of water tank one foot from ceiling.

No. 3. Directly exposed on top of blanket on floor.

No. 4. On top of wire dog cage four feet high between layers of woolen blanket—four layers on top and four layers underneath.

No. 5. Directly exposed on top of blanket on dog cage.
No. 6. Control. Kept under similar condition in another room.
Gas generated with one machine from the exterior.
Room remained closed 16 hours.

RESULTS.

Aureus.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—
6	+	+	+

Typhoid.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—
6	+	+	+

Diphtheria.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—
6	+	+	+

Anthrax.

	24 hrs.	48 hrs.	72 hrs.
1	—	—	—
2	—	—	—
3	—	—	—
4	—	—	—
5	—	—	—
6	+	+	+

EXPERIMENT XX.

Machines—Type II. Gas generated from formaldehyd solution. At the same time evaporation of formaldehyd solution from blankets suspended in stable.

Weather—Temperature, 47½° F. Humidity, 73. Rain at intervals.

Cultures—Aureus, typhoid, colon and tubercle bacilli.

Room—Large weather-stripped stable containing 18 windows and 14 doors, cement and ground floor, ceiling of tongued and grooved boards. All windows closed tightly and cracks of doors sealed with heavy manilla paper. Capacity, 56127 cubic feet.

Formaldehyd solution—640 ounces used—one-half in machines and one-half on blankets. (Rate, 11.4 ounces to 1000 cubic feet.)

Method—320 ounces of the formaldehyd solution, with 1 per cent. glycerin, was divided equally among four machines, each being filled twice, and operated from the exterior. Eight blankets were saturated with the balance of the formaldehyd solution after the addition of one-third water and suspended on wires at different heights throughout the stable.

The stable was at once tightly closed and the machines started, requiring four hours for the generation of the gas.

DISTRIBUTION OF CULTURES.

No. 1. Between four layers of towel on feed wagon four feet high.
No. 2. Directly exposed on floor.
No. 3. Directly exposed on feed wagon four feet high in centre of stable.
No. 4. Directly exposed on top of mangers and on steps at different heights.
No. 5. Control. Kept in same manner in another building.
Room remained closed for 36 hours before removal of cultures.

RESULTS.

	Aureus.				Typhoid.				Colon.		
	24 hrs.	48 hrs.	72 hrs.		24 hrs.	48 hrs.	72 hrs.		24 hrs.	48 hrs.	72 hrs.
1	—	—	—	1	—	—	—	1	—	—	—
2	—	—	—	2	—	—	—	2	—	—	—
3	—	—	—	3	—	—	—	3	—	—	—
4	—	—	—	4	—	—	—	4	—	—	—
5	+	+	+	5	+	+	+	5	+	+	+

Only the No. 2 method of exposure used for the tubercle bacilli. The strips of paper containing bacilli were immersed in sterile water for several hours. After being well shaken two guinea pigs were inoculated intraperitoneally with the suspension, which were killed at the end of eight weeks showing no lesions of any character.

The third method employed was forced on us by being confronted by the necessity of disinfecting an enormous stable belonging to one of our largest brewing companies, in which an epidemic of so-called cerebro-spinal meningitis had appeared. The stable proper contained 235,000 cubic feet of space, while the loft contained 180,000. The work was done in two parts, the loft being done first. After sealing the openings as well as possible, sixteen blankets were saturated with a solution of two parts formalin (40 per cent.) and one part water, and then suspended over ropes, care being taken to distribute them evenly. The amount of formaldehyd solution (40 per cent.) used was 130 pounds, to which 43 pounds of water was added.

Temperature 70. Humidity 38. June 17, 1902. Clear day.

The staphylococcus pyogenes aureus was used as a test organism, the method of preparation being the same as already described.

Distribution of cultures:

1. On top of a pile of baled hay, 14 feet from the floor.
2. In feed room under a layer of cut feed 1 inch deep.
3. On loose hay 3 feet from floor.
4. Under layer of loose hay 1 foot thick, and 4 feet from floor.
5. On floor exposed.
6. Stuck under edge of bale of straw 2 feet from floor.

The loft was kept closed for 43 hours, and when opened the gas was still very strong.

Result: None of the cultures showed growth after 72 hours.

The disinfection of the stable was carried out in the same manner 15 days later. The same amount of formaldehyd solution was used distributed through the stable on seventeen blankets, which were hung over the stall partitions.

Temperature 75° F. Humidity 50. July 2, 1902. Clear day.

The staphylococcus pyogenes aureus was again used as the test organism.

Distribution of cultures:

1. In feed box four feet from floor.
2. Under floor in drain covered by grating.
3. On box in center of room eight feet from floor.
4. On floor.
5. On steps at side of room six feet from floor.

The stable was kept closed for 24 hours, and when opened the odor of the gas was strong.

Result: None of the cultures showed growth after 72 hours.

The construction of the stable no doubt assisted in the good results obtained, being well built of brick with a slag roof. Whether such results could be had in cold and windy weather is a question which can be determined only by experiment, but it seems doubtful. It will be observed that in the loft the amount of formaldehyd solution used was slightly in excess of 10 oz. per 1000 cubic feet of space, as usually recommended; while in the stable the amount was less than this. The experiment is instructive in showing that large spaces can be disinfected without the use of expensive apparatus, it being practically out of the question for the average person to obtain a sufficient number of generators of any pattern to fill such large enclosures with gas.

In reaching the following conclusions we have taken into consideration not only the experiments reported here, but a considerable number of others done by ourselves with other types of apparatus, as well as numerous ones reported by workers in this country and in Europe.

CONCLUSIONS.

1. Formaldehyd is justly entitled to the high position which has been given to it as a disinfectant.

2. Special apparatus, while useful and convenient, is not absolutely necessary for the successful application of the gas.

3. The germicidal power of formaldehyd gas is dependent on certain factors which are as yet imperfectly understood. Other things being equal moisture and temperature are the most important of these factors.

4. In practice every operation should be controlled by cultural experiments, and no room which has been exposed to infection should be considered as disinfected unless control cultures exposed in various parts of said room are shown to have been destroyed.

THE METHOD OF TRANSMISSION OF YELLOW FEVER FROM MAN TO MAN.

By W. C. GORGAS, M. D., U. S. Army.

Is yellow fever transmitted by fomites alone, by the *Stegomyia* mosquito alone, or by both? Or if by the mosquito alone, can the mosquito become infected in any other way than by biting a sick man?

Since yellow fever began to attract general attention as a distinct disease, about the latter part of the seventeenth century, its mode of propagation has been widely discussed. The belief has been quite general at various times, and held by practitioners of weight and distinction, that it was an intensive form of malarial fever; and by others, that it was a directly contagious disease, propagated from person to person, like smallpox, and by others, that it was in no way communicated from person to person, but due to local conditions of heat, temperature and filth that generated a miasma, such as was generally believed to be the cause of malaria.

At present, opinion is much divided as to its method of propagation. Until very recently, it was universally believed that, while it could not be generated *de novo* and had always to come from some preceding case of yellow fever, it was transferred from this case by the living germ. This living germ could either be transferred directly from the sick to the well, or through the means of fomites. This is practically believing the disease to be contagious. Others believed that the germ could not be directly transferred, but after leaving the body of a sick person, had to undergo some further stage of development.

During the last two years it has been accepted by a considerable number of the profession that the disease, though due to an unknown germ, was only transferred by a single species of mosquito, and that the germ could not live outside of the body of either the mosquito or man. This is a question of great practical importance. If the disease can be conveyed by germs which can be transferred in fomites of various kinds, there is a possibility of the disease being introduced into a community by fomites coming from any infected region. This necessitates the destruction of these germs by disinfecting all ships and cargoes coming from points where they might have been infected, and the disinfecting goods of all kinds coming in any way through infected regions, and the detaining of persons who may by any possibility introduce the germ. If the germ can be only introduced in the body of man by the mosquito, all traffic can be left entirely free. The only sanitary measures necessary are the destruction of the infected mosquitoes, and the care of persons who may possibly transmit the germ.

The arguments on the side of fomites are very strong, and were universally accepted by the profession up to a very recent date. It seemed so obvious that this was the method of the transmission of the disease, that men who came in contact with it practically accepted the theory as proved. At first glance there seemed many cases in every man's experience that could only be explained on the theory of fomites, and there were many examples in the literature of yellow fever, which if established as facts, would prove beyond question that the disease was communicated by fomites.

The arguments, on the other hand, are based upon the experiments of the army board in Havana, who worked out the theory advanced by Dr. Finlay, of that city, that the *Stegomyia* mosquito is the only means of transmitting yellow fever. I will not go into these experiments at all; I think they are universally received, and it is generally acknowledged that this mosquito does convey yellow fever, the only difference of opinion being as to whether it is the only, or common means, of doing so.

Those who hold that it is the only means of propagating yellow fever do so for the following reasons: That the theory of fomites entirely fails to explain well-known and marked peculiarities of yellow fever. Take smallpox, or plague, or tuberculosis, which we all believe to be transmitted by fomites. We know that if fomites containing the germs of these diseases are transferred to any part of the world, in any season of the year, to any altitude, they may spread the disease, and will do so, if local conditions are favorable. It has been a well-known fact in the history of yellow fever, that it disappears when frost comes; that it never occurs in certain parts of the world, no matter how many or how often fomites are carried there; that it never occurs in certain altitudes, no matter how much infected material, or how many sick persons are carried there; that, in a general way, it is an inhabitant of the sea coast and low-lying lands. It is hard to explain these facts under the theory of fomites, but they are readily understood, if we believe that a single species of mosquito conveys the disease. It has never occurred in Asia or India, because they have been too far off and communication too infrequent and indirect between yellow fever centers and the points mentioned to enable a mosquito infected with yellow fever to live through the voyage. Fomites, however, must have been many times introduced into those regions, particularly into the English possessions in India and the Spanish possessions in the Philippine Islands.

It is not found in places of considerable altitude, because the *Stegomyia* mosquito cannot live in these altitudes. Take Veracruz and the City of Mexico; there is daily railroad communication between these points, and at the same time no check upon the introduction of fomites and passengers. Train loads of fomites are daily introduced into the City of Mexico from Veracruz, and many people come up from Veracruz and develop the disease in the City of Mexico. And dozens of similar instances could be given. This has been one of the well-known and striking facts in the history of yellow fever. If it were fomites, why should it not spread just as the fomites of smallpox or tuberculosis transmit those diseases? The mosquito theory entirely covers these cases. This species of mosquito cannot live in the City of Mexico. Why should frost or cold stop the spread of a disease propagated by fomites? Wherever man is, there would be always protected places in houses and dwellings that would give sufficient warmth for its development. Tuberculosis and smallpox, if introduced by fomites into the hut of an Esquimo, will give him the disease. Why not yellow fever? The mosquito gives a rational explanation of this; the *Stegomyia* cannot live in cold places or countries, but why should not the germs contained in fomites? All other infectious germs of which we have any knowledge can be transferred into a country, no matter how cold.

These peculiarities with regard to yellow fever called Dr. Finlay's attention to the mosquito twenty years ago. The experiments of the army board demonstrated that yellow fever could be transmitted by the mosquito, and under certain conditions, was always transmitted by the *Stegomyia*. For three or four months they had fomites of every conceivable kind and intensity of infection placed in such way as they thought best fitted to develop infection. Fresh non-immunes just from Spain were kept in this room in shifts, for a week at a time. In no single case was yellow fever contracted in this way. Certain of these non-immunes were afterwards taken and bitten by infected mosquitoes, and promptly had yellow fever. This we consider a strong argument of the fact that in Havana, at any rate, the disease was conveyed only by the *Stegomyia* mosquito, and if at all by fomites, only under very exceptional circumstances.

Acting on these lines, a city of 275,000 inhabitants, infected in every part, and with over 30,000 non-immunes present, was freed from yellow fever. No attention was paid to fomites of any kind. The only sanitary measures taken were those with reference to killing infected mosquitoes. During the year 1901, fomites from sick yellow fever patients were daily moved about the city, sent to the wash, and so on. Frequent trains every day came in from neighboring infected points, bearing fomites of every kind. Yet by killing infected mosquitoes alone, the city was freed from yellow fever.

It has been urged that this is not unusual in yellow fever, that many other cities have been freed from yellow fever before the mosquito theory was known. This is true of Boston, Philadelphia, New Orleans, Veracruz, Santiago and many other cities, but not of Havana. Yellow fever has never before been absent from Havana. It seems to me the man is certainly unreasonable, who, under the circumstances, hold that there is no relation of cause and effect between the killing of infected *Stegomyia* and the disappearance of yellow fever from Havana.

Many individual instances are introduced by the advocates of the fomites theory of the transmission of yellow fever that could only have been carried by fomites. It is very difficult to explain these cases; they were recorded generally at a time when there was no question as to fomites. The question in the mind of the recorder was not as to how this disease was conveyed; he knew it was conveyed by fomites in some way. The only question in his mind was how the fomites got there.

Dr. Souchon, President of the Louisiana State Board of Health, in an article in the Medical Record of February 8, 1902, quotes twenty-seven different cases which he thinks could not possibly have been conveyed by the mosquito, and could only have been conveyed by fomites; and Dr. J. Y. Porter, State Health Officer of Florida, in his report for 1901, quotes quite a number more. The negative side of a proposition is always a very difficult one to prove, whereas one case on the positive side settles the matter. Everybody who is practically conversant with yellow fever can recall hundreds and thousands of cases in which fomites failed to convey the disease. But if in any one of these cases it can be proved that the disease was yellow fever beyond reasonable doubt and that it was impossible for the disease to have been conveyed in any other way than by fomites, the mosquito side of the question is lost, and it is shown that the disease can be conveyed in other ways than by mosquitoes.

Take case 9 in Dr. Souchon's article:

9. Case of Mr. Lane, of Milton, Fla., 1853. Dr. Hulse, of Pensacola, Florida, reported the following instance:

"In 1855, a Mr. Lane was attacked with yellow fever at Dr. Fisher's in Milton (which is a few miles from Pensacola). His effects were packed in a trunk, which was locked, placed in a storehouse and covered with old cloth and sacks. In the summer of 1855, two years later, this trunk was sent from Milton to Brooklin, in Alabama, forty or forty-five miles north, and it was opened in a house in presence of several persons. Soon after, six of those living in this house sickened, and several died with black vomit."

The quotation is evidently a misprint in stating that Mr. Lane had yellow fever in 1855; the contents show that 1853 was intended.

Here was a case in which yellow fever was transmitted by fomites, and fomites two years old. If it could be proved that the secondary cases occurring in Brooklin, Alabama, were yellow fever, and that there was no yellow fever around Brooklin in 1855, and no possibility of an infected mosquito being introduced or getting at the patients, it would certainly prove that yellow fever could be transmitted by fomites.

Or again, take case 18:

"18. Case of Dr. Wm. Johnson, Surgeon United States Navy, 1860: On the termination of a cruise on the west coast of Africa, some years before, when yellow fever occurred on board the ship to which he was attached, he packed his books in a barrel and in that condition left them for about three months after they had been received and his arrival home. He then proceeded to unpack them, and three or four days thereafter he was taken with yellow fever and came near dying.

Yellow fever was apparently conveyed in this case by fomites which had been packed for three or four years. In this case, if the secondary disease was really yellow fever and there was no possibility of infected mosquitoes having gotten at the case, the proof would be complete:

Case 26:

"26. Case of the United States Steamer Plymouth, 1878: The persistence of yellow fever infection under conditions commonly fatal to mosquitoes is particularly well shown in the history of the United States steamer Plymouth. She first became infected at St. Thomas in the summer of 1878. In November following she was sent to Portsmouth, N. H., and about a month afterward to Boston where she remained four months. Meanwhile everything was removed from the ship; the crew were transferred and she was fully exposed to intense cold for a month—ten days of that time in dry dock—with a temperature at times below zero. The water in the tanks was frozen, as it was in basins put in the storerooms of the ship; and for several days after the fires were rekindled on board, the cold continued so intense that the water froze in the bilges at night. One hundred pounds of sulphur was burned below decks, the fumigation extending over two days; the berth deck, holds, and storerooms were thoroughly whitewashed with a mixture of lime and chloride of lime.

"March 15, 1879, she left Boston for a cruise in the West Indies. On the night of the 19th, in a severe gale of wind, the hatches had to be battened down, and the berth deck became very damp and stuffy. On the morning of the 22d (she had not touched at any port), Richard Saunders, machinist, was sick, but did not report until afternoon, when he had violent headache and fever. During the night of the 22d, Peter Eagan, boatswain's mate, was taken suddenly and violently sick with the same symptoms. On the 23d, at sea, latitude N. 27 degrees 48 minutes, longitude W. 57 degrees 59 minutes, the surgeon (Woolverton) reported both cases yellow fever, and recommended that the ship proceed no further on that cruise. The first case recovered; the second died on the third day of illness. The ship's course was changed to northward, and she arrived at Portsmouth, N. H., April 7. No new case appeared after she turned. 'I am convinced,' said the surgeon, in his report, 'that the fever infection is confined to the hull of the ship, and especially to the unsound wood about the berth deck; and that while the Plymouth is a safe and healthy ship in a temperate climate, she cannot, for the present, be sent into a tropical climate without the certainty of an outbreak of yellow fever occurring on board; and that no such precautionary measures whatever could avail to avert such a result.'

"Soon after her return to Portsmouth, April 7, she was surveyed by a board of medical officers, who reported that: 'After careful consideration of all the facts, we have arrived at the conclusion that the infecting virus which gave rise to the last outbreak of fever must have been preserved in the rotten wood of the berth deck or in the bedding or clothing of the crew.'"

This would be equally convincing. In this case we have gotten the complete report from the office of the surgeon general of the navy, and think that the secondary disease, if it were yellow fever, could be more readily explained under the mosquito theory than under the theory of fomites.

I quote these three cases as probably the strongest mentioned in favor of fomites. But it does not seem to me that these cases should at all bear the weight that the scientific experiments made by the board, of which Major Reed was president, carry.

On the other hand, the work of the army board is advanced as proving that mosquitoes can convey yellow fever. These and other cases in yellow fever literature are advanced as proof that fomites can transmit yellow fever. For a scientific proof on either side, it is necessary that it be proved that the disease produced was yellow fever, and that it could only be transmitted by the means alleged, and in no other way. In the cases reported by the army board, every precaution was taken to verify the diagnosis. The cases were seen by many practitioners in Havana who had no connection with the experiments, and each case was examined and passed upon by the official board of the sanitary department. On this board, among others, were Dr. Carlos Finlay and Dr. Juan Guitéras. On the other side are brought up a number of cases which it is asserted could only have been transmitted by fomites. The three quoted are, I think, the strongest.

Take case 18: If it were proved that the disease contracted by Dr. Johnson were yellow fever, and that it occurred in a part of the country and under such circumstances that it would only have been conveyed by fomites, it would certainly prove that fomites could convey the disease. But compare the cases; see how weak such evidence is compared with that on the side of mosquito transmission. It was, no doubt, a very good record at the time at which it was made, and very conclusive evidence at that time that yellow fever could be conveyed by fomites and could remain dormant for many years. Then, no question was raised as to fomites conveying yellow fever. All thought they knew that it was conveyed in this way and the only question in reporting these cases was some peculiarity in this particular method of conveyance.

Let us now consider the evidence as to whether the disease could have been conveyed only in the way in which alleged, by mosquitoes on the one hand, or by fomites on the other.

The army board so arranged their cases that it was scientifically impossible for the disease to have been given except by the mosquito. The experiments were carried on in an isolated locality; the subjects were quarantined in this camp, and the whole arrangements were such that all who saw the experiments or read the reports have acquiesced in the decision that the experiments were scientifically accurate, and that it was impossible for the disease to have been conveyed except by the mosquito, in these particular cases.

On the other hand, are brought up many instances from the literature of yellow fever during the last century, in which it is alleged that fomites only could have conveyed the disease. In many of them we can see from the cases as reported that the possibility of the disease being conveyed in some other way than by fomites could not be excluded, as they occurred in sections where yellow fever was prevailing.

Take such a case as number 1, transmitted from Centerville to Franklin, five miles apart. As yellow fever had been going on all during the summer at Centerville, it seems impossible with any scientific accuracy to exclude the possibility of the disease having been conveyed by the mosquito directly or indirectly. Such instances, with our present knowledge, would probably not be recorded as cases of transmission by fomites. Nor would the other three cases quoted, in which the yellow fever lay dormant in fomites for from two to six years, and then spread the disease. In Havana we tried to transmit the disease by fomites, under circumstances in which other methods of transmission were absolutely excluded. This was done both by the army board, and at the experimental station of the sanitary department at Las Animas Hospital, by Dr. Ross, with some fifteen or twenty individuals known to be non-immune to yellow fever. But none of the cases sickened in any way.

I dare make a prediction, that there will be very few cases brought forward as having occurred, from now on, of the transmission of yellow fever by fomites. I do not mean to make any insinuation as to the honesty of the persons making the report of cases heretofore quoted, as evidence of the transmission of yellow fever by fomites, but simply that they were recorded at a time when the state of our knowledge of this disease did not require any greater accuracy than was given in most of these cases.

And now, as to whether the mosquito could become infected in any other way than from the yellow fever patient: To my knowledge no direct experiments have been made in this direction, but in Havana during 1901, the mosquitoes had the same access as during former years to all other sources of infection, except the sick man. By excluding the mosquito from the sick man and killing those which had become infected in this way, the city was cleared of yellow fever. If the mosquito could have become infected from some source not known, or from infected clothing and fomites of various kinds, it had the same access to those as in former years, and our measures would not have been successful in eradicating yellow fever.

To sum up: The advocates of the theory that yellow fever can be conveyed by fomites advance as an argument many cases recorded during the last century, in which they assert that yellow fever could only have been conveyed by fomites. The advocates of the mosquito doctrine advance the experiments of the army board at Havana, in which it was scientifically demonstrated that in the particular cases brought forward by them, yellow fever could not possibly have been conveyed in any other way than by the mosquito. They bring forward the experiments of the army board, and those at Las Animas Hospital, with fomites, in which the disease was not conveyed. They bring forward the eradication of yellow fever from Havana, in which attention was given only to infected mosquitoes, and in which fomites were transferred from place to place about the city, and freely admitted from the outside. It is a question, I think, that will very soon be settled one way or the other. The methods that were successful in Havana will surely be tried in places like Veracruz and Rio Janeiro, and if successful there, will settle the question. And if the disease can be conveyed by fomites, certainly instances will soon be found where this is evident, and the matter settled on that side.

Our government is about to undertake the construction of an Isthmian canal, and the measures that have proved successful at Havana with regard to yellow fever and malarial fever are to be adopted there, as far as practicable. If we can reduce or do away with the excessive mortality that has always heretofore occurred upon the introduction of any large body of non-immunes into the Isthmus, this will be another object lesson in favor of the mosquito doctrine, both with regard to malaria and yellow fever.

REASONS FOR BELIEVING THAT THE ONLY WAY IN NATURE[1] FOR YELLOW FEVER TO BE CONTRACTED BY MAN IS FROM THE MOSQUITO.

BY JOHN W. ROSS, M. D., SURGEON U. S. NAVY.

This article is intended mainly for the busy medical man, who from want of time and lack of opportunity, has not kept up with the details of the remarkable revolution which has taken place during the last few years in our knowledge of the etiology and prevention of yellow fever.

Havana, the chief focus and home of yellow fever, was constantly afflicted by that disease, never a month without deaths from it, from 1762 to September, 1901, a period of about 140 years. Eight months subsequent to the beginning of the American anti-mosquito sanitary work, which was based entirely upon the mosquito idea of the propagation of that disease, yellow fever had disappeared altogether from Havana, and since, up to the present time—an interval of 14 months, embracing two autumns and a summer—not a single case of the disease has occurred in that city. The intervening summer was an unusually hot one, and the city contained an unusually large number (at least 40,000) recently arrived non-immunes—just the conditions, which under the former regime must inevitably have produced a severe epidemic of yellow fever.

Sanarelli's proposed solution of the yellow fever problem, of which we had such high hopes four or five years ago, has been proven untenable by numerous high authorities and skilled investigators. The United States Army Yellow Fever Commission, composed of Major Walter Reed and Drs. Carroll, Lazear and Agramonte, United States Army, which visited Havana first in the early summer of 1900 investigated its claims and found that:

"First—The bacillus icteroides, Sanarelli, stands in no causative relation to yellow fever, but when present should be considered as a secondary invader in this disease.

"Second—The mosquito serves as the intermediate host for the parasite of yellow fever."

This commission returned to the United States the same summer, two of its brave members having had yellow fever, from which one of them, Dr. Jesse W. Lazear, died, thereby adding one more noble name to the long roll of medical martyrs who have yielded up their lives trying to help their fellow-men.

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[1] Yellow fever can with great certainty be produced by inoculating non-immunes with blood taken from persons affected with that disease; but, for obvious reasons, this cannot be called a natural method.

The latest utterance I have read about the B. icteroides, that of Dr. Porthier, a member of the recent United States Public Health and Marine-Hospital Commission on the "Investigation of the Yellow Fever and Mosquitoes in Veracruz," is, in part, as follows:

"The B. Sanarelli was doomed. The work of Reed and Carroll as to the transmission of yellow fever by the *Stegomyia fasciata* has been entirely corroborated." (Jour. Am. Med. Asso., Nov. 8, 1902, p. 1207).

In 1881, Dr. Carlos J. Finlay, a scholarly scientist, a British subject, born in Cuba, and a resident of Havana, promulgated the theory (now an established fact), of the transmission of yellow fever by the mosquito, and singled out the guilty species, now called *Stegomyia fasciata*. For many years subsequently he conducted experiments and published numerous articles attempting to convince the medical profession of the correctness of his views. In one of his contributions, read at Buda-Pesth in 1894, he recommends the following measures against the propagation of yellow fever through mosquitoes, which from our present point of view we may call prophetic:

"First—To prevent those insects from stinging yellow fever patients.

"Second—To destroy as far as possible the mosquitoes which have been infected. * * *

"Third—Finally, to consider any place unsafe so long as the last mosquitoes which have stung yellow fever patients may be alive in it." * * *

The beginning of this philosophic work of Finlay antedated by two or three years any of the mosquito-malarial theories and demonstrations. It occurred about the same time (but altogether independently) as the discovery by Sir Patrick Manson of the transmission of the *filaria nocturna*—the worm which causes *elephantiasis arabum*—by mosquitoes.

The brilliant work of Ronald Ross—1895-1899—Grassi, Marchiafava, Celli, and others, especially that of the first-named scientist, establishing the cognate fact of the conveyance of malaria by the *Anopheles* mosquito had great influence in leading to the demonstration of the transmission of yellow fever by mosquitoes.

The clear-cut scientific observations and classical contributions of Surgeon H. R. Carter, United States Marine-Hospital Service, showing the interval (about two weeks) which occurs in houses between the first (infecting) case and the first group of cases secondarily infected; so interesting and impossible to explain by the fomites theory, are now readily understood in the light of our present knowledge of the elaboration of the yellow fever poison in the body of the mosquito—that process requiring not less than 12 days.

The Army Medical Commission, presided over by Walter Reed, First Vice-President of this Association for the present year, whose recent untimely death—an incalculable loss to the medical profession and to humanity—we are now mourning, returned to Cuba in the autumn of 1900, and resumed their mosquito-yellow-fever investigations. The reasons given by Major Reed for taking up the mosquito problem were:

1. Finding that Sanarelli's bacillus had nothing to do with the causation of yellow fever.
2. The unsatisfactoriness of the proof of the propagation of the disease by fomites.
3. The perfect demonstration by Ronald Ross, and the Italian observers, that the *Anopheles* mosquito is the intermediate host and conveyer to man of the malaria parasite.
4. Carter's establishment, published in May, 1900, of the two to three weeks' interval between the first (infecting) case of yellow fever in a house or ship and the subsequent outbreak or group of cases.

This Commission's subsequent work continued during the remainder of the autumn, and following winter, 1900-01.

They produced 13 cases of yellow fever by having non-immunes stung by contaminated mosquitoes (*Stegomyia fasciata*). Three other cases were produced by direct inoculations of the blood of yellow fever patients.

These experiments were conducted in such a manner as to make it impossible for the patients to have contracted the disease in any other way. Each case was diagnosed positively as yellow fever by the official Yellow Fever Commission of Havana, composed of such eminent experts as Finlay, Gorgas, Guitéras and Albertini; whose function was to make diagnosis of all cases reported as suspicious of being yellow fever.

The famous paper read by Major Walter Reed before the Pan-American Medical Congress, Havana, February, 1901, describes their flawless procedures, which leave no possible room for doubt that yellow fever is contracted by man from the mosquito. Nevertheless, the medical profession of Havana remained unconvinced, and similar experiments, with similar results, were repeated during the succeeding year inside the city of Havana, at Las Animas Hospital, of which I was at the time the director. An "Inoculation Station" was established in said hospital, under the immediate charge of Dr. Juan Guitéras. Eight experimental cases, in all, were produced at Las

Animas. The diagnoses were verified by the Yellow Fever Commission of Havana. During their illness the patients were unrestrictedly visited by the leading physicians of Havana.

Thus it will be seen that 21 well-marked cases of yellow fever—Reed's 13, and Guitéras' 8—representing the different varieties and intensities of the disease, were unquestionably caused by the bite of contaminated mosquitoes, within the established incubation period (one to six days) with almost mathematical precision. Three of these mosquito-borne cases proved fatal, and the necropsy revealed the characteristic lesions of yellow fever. The demonstration of the fact that yellow fever is produced by the mosquito was then, for the first time, accepted as complete by the physicians of Havana.

It now remains to consider whether or not yellow fever is capable of being produced in nature in any other way. This question is so important, and doubt in regard to it so natural, that I shall give in detail two sets of experiments—one conducted for its elucidation at Quemados, by the United States Army Commission; the other, at Las Animas Hospital, by myself. In these two sets of experiments every possible effort was made to cause yellow fever by fomites, the result in each instance being a signal failure.

"There was erected at Camp Lazear (one mile from Quemados, and about seven miles from Havana), a small frame house with a cubic capacity of about 2800 feet, tightly ceiled and battened, provided with small windows, so as to prevent a thorough circulation of air through the house, and with wooden shutters to prevent the disinfecting qualities of sunlight. The windows were closed by permanent wire screens with a 5-millimetre mesh. The vestibule was protected by a solid door and a wire screen door, and the inner entrance by a second wire screen door. In this way the passage of mosquitoes into the room was effectively excluded. The average temperature was kept at 76.2° F. for a period of 63 days, and precaution was taken to maintain sufficient humidity of the atmosphere. On November 30th, three large boxes filled with sheets, pillow-slips, blankets and so on, contaminated by contact with cases of yellow fever and their discharges were received and placed in the room. Most of these articles had been taken from the beds of patients sick with yellow fever in Havana. Many of them had been purposely soiled with the excretions of patients. These soiled sheets, pillow cases and blankets were used in preparing the beds in which the members of the hospital corps slept. During 63 days this building, thus furnished, was occupied by seven non-immune persons. Three of them occupied the room each night for 20 days. Later, a fourth box of clothing and bedding was added, some of it purposely soiled with the bloody stools of a fatal case of yellow fever. Two of them occupied beds from December 21st to January 10th, every night wearing the very garments worn by yellow fever patients throughout their entire attacks, making use exclusively of their much soiled pillow-slips, sheets and blankets. At the end of 21 nights they went into quarantine (as had the former three) and were released five days later. Two other non-immunes occupied the same beds for 20 days more. The attempt, therefore, which was made to infect this building and its seven non-immune occupants during a period of 63 days was an absolute failure; all seven were released from quarantine in excellent health."—From "Mosquitoes," by Howard.

Later on some of these same men were bitten by yellow fever mosquitoes, and promptly came down with yellow fever.

These fomites experiments have been pronounced inconclusive by many medical men upon the grounds, chiefly, that they were conducted during the winter season, and in the country, outside the city.

The following extract from my "Report of Experiments with Yellow Fever Fomites at Las Animas Hospital, Havana, Cuba, September-November 1901" read before the International Sanitary Congress, Havana, February 10, 1902, sets forth the principal details and results of the second set of experiments, in which the objections urged to the first set are removed.

"Two rooms were selected in buildings constructed of masonry, about 150 meters apart, both within the hospital enclosure. These rooms were thoroughly renovated, provided with doors and windows of wire gauze—mosquito proof—being further protected by vestibules of the same material. All the cracks and rough places in the walls, ceilings and floors were smoothly sealed; and an abundance of pyrethrum powder burned in the rooms, in order to preclude the possibility of the presence of any mosquitoes. One of these rooms was called the observation room, and the other, the experimental room.

The observation room was fitted out with furniture, utensils, bedding and clothing which had been thoroughly disinfected, so that there could be no suspicion of extraneous contamination.

In the experimental room were placed numerous bundles containing clothing, bed clothes and bedding which had recently been used in the sick rooms and on the persons of patients ill with yellow fever; much of it badly soiled with discharges and excreta of said patients. These articles had been used by Venturo Trillo Ramos and Juan Lago, at the private hospital, "La Benefica," Havana (usually an infected place) and by patients in the hospital at Columbia Barracks, Quemados, Cuba. All these cases had been diagnosed as yellow fever in their respective hospitals, and confirmed as such by the yellow fever commission of Havana. The cases of Juan Lago and Vicente Real were fatal, the latter having presented during its course and at the autopsy unmistakable appearances of malignant yellow fever.

Eight men, newly arrived on the island, five Spanish, two Italian and one English, all healthy, by no possibility immune were procured from the immigration station, Triscornia, across the bay from Havana, as subjects for our experimentation.

Two of these men were placed in the observation room on September 27, and confined there for more than a week. At the end of that period they were transferred to the experimental room, where they were kept for seven days. They were then released and given light work in the hospital grounds. When they were removed from the observation room, two others of the subjects were placed therein for seven days—afterwards taking their turn in the experimental room for the same length of time. In this way, all eight of the men, after rigid physical examination, went through the observation and experimental rooms, two by two, emerging therefrom in good health, and remaining so until all possibility of having contracted yellow fever had passed away.

While in the experimental room the subjects used, upon their persons for sleeping, the night shirts, pajamas, bedding, bed clothes, etc., above described, soiled with fecal dejections, black vomit and blood. From time to time other packages, containing similarly soiled fomites, were introduced and the subjects required to frequently shake them within the room. While in the observation room, the men were not allowed to leave it, except for necessary exercise, and then under guard. While in the experimental room, they were not allowd outside under any condition. Their food was carried to them, and there were guards over them night and day.

A careful clinical record was kept of each man during the 14 days he was kept in the observation and experimental room. The temperature, pulse, respiration, appetite, etc., of all eight continued practically normal throughout.

At the beginning of these proceedings, written invitations were sent to the physicians of Havana, requesting them to visit Las Animas at their pleasure, to observe the progress of the experiments.

I beg leave to call especial attention to the following facts in connection with these experiments, viz.:

1. They were conducted in the city of Havana itself, within whose beautiful limits, until recently, Cuba's yellow sorrow had run riot continuously for 139 years.

2. They were conducted in buildings, similar in construction and material (mamposteria) to those occupied by people of the poorer classes of the city—damp, poorly lighted, badly ventilated, floors on the level of the ground.

3. They were conducted at the season of the year when, in this latitude, the dreaded scourge is at its worst and does its most deadly work." Said period comprising the latter part of September, the whole of October, and the first part of November—the height of the yellow fever season at Havana.

Military Governor Wood, with his characteristically clear perception, appreciated, from its very beginning, the promise and importance of the mosquito-yellow fever work in Havana. He generously provided the funds for the experiments and for the large expenditures, sometimes $35,000 a month, subsequently and consequently required by Major Gorgas, the chief sanitary officer, for the successful conduct of the anti-mosquito crusade. Major Reed, in the paper referred to above, expresses sincere thanks to "Major General Leonard Wood, U. S. V., without whose approval and assistance, these observations could not have been carried out."

For 140 years prior to the late Spanish-American war, Havana had never been free from yellow fever; the annual number of deaths from that disease having averaged from 500 to 1000, some years as many as 1600—largely among the Spanish soldiers, great numbers of whom were imported every year, up to 1898. During the first two years of American occupation the sanitary authorities devoted themselves energetically to putting the city in good hygienic condition, and especially to measures then considered most effective against yellow fever; such as the destruction and disinfection of fomites and localities supposedly infected by that disease. The result was a rapid decrease of all infectious diseases, except yellow fever, and an enormous diminution of the general mortality rate. Smallpox, which had been a terrible scourge under the Spanish administration, disappeared entirely in July, 1900, and has not returned since. Nevertheless, the amount of yellow fever in the city did not diminish along with other fevers, showing that ordinary sanitary measures had no effect upon it. There was more yellow fever in Havana during December, 1899, than in any December for several previous years. In 1900, there was a sharp epidemic, and in December of that year, there were 20 deaths from yellow fever in the city. In January, 1901, there occurred 24 cases in Havana. In February, 1901, Major Gorgas, having fully accepted the demonstration of the propagation of yellow fever by mosquitoes, turned all the energies of himself and his department to,

First—The protection of yellow fever patients from mosquitoes.

Second—The prevention of the infection of mosquitoes by yellow fever patients.

Third—The destruction, by fumigation, etc., of all possibly infected mosquitoes. And

Fourth—The abolition of all water barrels, pools and other breeding places of mosquitoes in Havana.

Practically no attention was paid to fomites, of which at the time there must have been a great abundance in the city. No attempt was made to get rid of fleas and bed-bugs, by which Havana is, at all times, infested. The result of this concentration upon the mosquito, and neglect of every other supposed cause of yellow fever, was a rapid subsidence of that disease until within less than eight months, the last case had occurred in the city, after which yellow fever disappeared entirely. Not a case of it has occurred in Havana since September 27, 1901; although during the summer and autumn just ended patients sick with yellow fever, contracted in Mexico and Central America, were frequently carried through the heart of the city from the ships by which they had been imported, to the yellow fever hospital. During their transportation from ship to shore, and along the streets, they were kept carefully covered with mosquito netting, and upon arriving at the hospital, were placed in wards made mosquito-proof by wire-gauze screens. In the same wards were treated, at the same time, non-immune patients, sick with other diseases, and they were attended by non-immune nurses. That yellow fever is contracted solely from the mosquito is now so firmly and universally believed in Havana, that no one has the slightest fear of patients affected with that disease, provided the said patients be protected from mosquitoes and the mosquitoes from them.

It is contended by the opponents of the mosquito doctrine that the disappearance of yellow fever from Havana may not have been due to Major Gorgas' active anti-mosquito measures; because yellow fever had frequently disappeared, for considerable periods, from Santiago and other Cuban cities. But the conditions were quite different in Havana and the smaller cities. In the latter, the number of non-immunes was always very small, and the epidemic of yellow fever in one of these soon ceased, on account of the consumption of susceptible material; whereas, in Havana, the commercial, military, and yellow fever center of the island, the number of non-immunes was always large, thousands of immigrants coming into it every year, constituting an uninterrupted and inexhaustible stream of fresh material for keeping up yellow fever in an epidemic form.

All of the many able, cultivated, trained American medical officers —among them such men as General Wood, Surgeon-General O'Reilly, Major Kean, Major Gorgas, Surgeon Carter, Surgeon Glennan, Captain Stark, and others, who were in Havana, and who had personal knowledge of the mosquito-yellow fever experiments and demonstrations made there, were, without exception, completely convinced that *yellow fever is transmitted to man exclusively by the mosquito.* This is exceedingly strong evidence, to my mind, of the correctness of the proposition stated in the title of this paper. Had our distinguished colleagues, who do not agree with us, been on the ground as we were; and seen with their own eyes, as we saw, the exactness and completeness of the proofs, I believe they would have been convinced, as we were.

The idea, held by some able and prominent medical men, that yellow fever may be transmitted by bed-bugs, fleas and other suctorial insects, is, in the light of our present knowledge of insects and disease, highly improbable. Bed-bugs and fleas are found all over the world, in all climates, and at all seasons of the year; whereas, yellow fever is restricted to certain tropical and sub-tropical regions, and prevails only in the summer and autumn.

Another highly significant fact, in this connection, is that while yellow fever was melting away like ice under a summer sun before Major Gorgas' anti-mosquito operations, the city of Havana was infested with bed-bugs, fleas, etc., galore; no attention being paid to them by the sanitary department, and that the same prevalence of these blood-suckers has continued throughout the subsequent 14 months, during which the city has remained free from yellow fever.

Although the germ of yellow fever has not been found (no more has that of smallpox) yet, the scientific and circumstantial evidence is practically positive that it is an animal parasite, whose cycle requires a certain length of time, and is very similar to that of the malarial parasite. We know that the yellow fever germ goes from man to the *Stegomyia* mosquito and from the *Stegomyia* back to man. No instance has ever been advanced in which there seemed to me even a possibility that yellow fever had been transmitted by bed-bugs, fleas or flies. Certain it is that no such case has occurred in Havana during the past 14 months, in which yellow fever has been absent from that city, although, as shown above, unequalled opportunities for such an occurrence have there been constantly in existence.

Carter, in a very able contribution (Yellow Fever Institute Bulletin, No. 11) sums up as follows:

"For the belief that yellow fever is conveyed in nature *only* by a host, and doubtless a mosquito host, we have,

"First—The analogy of other diseases conveyed by insect hosts.

"Second—That all facts observed about the propagation of yellow fever agree with the necessary deductions of this theory.

"Third—No other theory explains all the facts observed of its propagation."

The fact that so many, perhaps all, of the peculiarities and problems of yellow fever, impossible to understand formerly, are explainable and capable of solution by what we know of mosquitoes and their relation to disease, is highly interesting and important. A few of these are:

Its singularly restricted area; its dislike for altitude, upper stories, plateaus, mountains, etc.; its partiality for the sea coast and low-lying districts; its incommunicability from man to man; the meaning of "place infection"; the interval, two to three weeks, between first (infecting) case and subsequent group of cases (Carter); its frequent appearance at sea several weeks after vessels had left port; its arrest by frost and cold weather; its survival through winters; its absence from Asia and India, etc.

Of the problems elucidated by the discovery of the mosquito origin of yellow fever, I cite that of the knottiest of them all, *the second outbreak on board the U. S. S. Plymouth, in 1879* (utterly inexplicable to me under the fomites, or any other theory). The *Plymouth* sailed from New England early in October, 1878; arrived at St. Thomas, W. I. (a yellow fever stronghold), October 21st, where she went alongside the wharf and coaled ship, remaining until October 25th. The officers and stewards were allowed to go on shore during this time. On the 25th, she went to Frederickstadt, Santa Cruz. November 4th, while at Frederickstadt, a case of yellow fever appeared on board. Between the 4th and 7th, seven cases occurred on board. On the 7th, the *Plymouth* started north, after which no fresh cases occurred in the ship. December 1st, she reached Portsmouth, New Hampshire, where the weather is usually cold at that season of the year. From Portsmouth she went to the nearby city of Boston, where she was emptied of her crew and stores, opened up and frozen out during January and a part of February, 1879. The vessel had been several times fumigated with burning sulphur. February 12th, 1879, she was recommissioned, and, on March 15th, started back to the West Indies. March 19th, "the damp berth-deck became very warm; a tropical condition prevailed." March 21st, immediately after reaching the tropics, and before she had touched at any port, yellow fever broke out among her crew.

The solution afforded by the mosquito doctrine is simple and satisfactory. The *Plymouth* left the West Indies November 7, with several fresh yellow fever cases, and, doubtless, with numerous contaminated mosquitoes on board. So soon as she entered winter weather, about December 1st, the infected mosquitoes went into hibernation and remained torpid until they re-entered the tropics, when some of them became active and stung certain of the men, thereby giving two of them yellow fever.

That mosquitoes hibernate is a well-known fact. Theobald, in his standard monograph, states that mosquitoes hibernate in the extreme cold of the arctic regions; that the gravid females seek shelter as soon as the weather becomes cold, lie dormant all winter, and deposit their eggs in early spring. Howard, the highest American authority upon the subject, says in his capital work, "Mosquitoes," page 6: "Mosquitoes hibernate in the adult condition." In conversation with this distinguished entomologist in Washington the other day (November 22, 1902) Howard used these words, which I took down verbatim: "The *Stegomyia* hibernates in the adult stage. I have a strong suspicion that it hibernates in the egg stage also."

Too much admiration cannot be accorded to Major Gorgas for the superb manner in which he accomplished the deliverance of Havana from yellow fever. In planning and conducting this creative work, without any precedent to guide him, he showed not only consummate administrative and executive ability and foresight, but, also, genius of a high order. After it was finished, it was impossible for us to look back upon it and point out any particular in which the work could have been improved.

At the recent meeting of the Conference of State and Provincial Boards of Health of North America, at New Haven, Conn., it was resolved concerning Major Gorgas' sanitary redemption of Havana, that, "we regard (it) as one of the most brilliant achievements of the application of sanitary science to public health work ever accomplished."

REPORT ON YELLOW FEVER IN THE REPUBLIC OF MEXICO, FROM OCTOBER 31st, 1901, to SEPTEMBER 30th, 1902.

By Dr. E. LICÉAGA, Mexico, Mex.

I shall continue, as I have done in former years, to report this time to the American Public Health Association concerning the yellow fever cases observed in different places of the Mexican territory, from October 1st, of last year, to September 30th, of the present year.

In the State of Veracruz there have appeared 877 cases during the period referred to, distributed as follows: Seven hundred and twenty-one at the port of Veracruz, ninety-two at the port of Coatzacoalcos, thirteen in the city of Cordoba, one at the port of Tuxpam, three at the port of Alvarado, eighteen in the city of Orizaba, one at Acayucan, one in the station called Juanita A. Perez (Cosamaloapam) and twenty-seven at Jalapa.

The said twenty-seven deaths occurred in the city of Jalapa, but the said city has been free from the disease up to the present time. All these deaths refer to patients who contracted the disease at Veracruz.

In the State of Yucatan only three cases were recorded, which occurred at the port of Progreso. In the State of Campeache, only one case occurred at the port of Laguna del Carmen. In the State of Tabasco occurred eight cases in the city of San Juan Bautista, and one at the port of Frontera; and in the State of Tamaulipas three cases were noted at the port of Tampico.

The states just mentioned belong to the coast of the Gulf of Mexico.

On the coast of the Pacific ocean three cases of yellow fever were noted in three of the states bordering on said coast, which cases were distributed as follows:

State of Oaxaca: Fourteen cases at the port of Salina Cruz, eight cases at Tehauntepec, one case at San Geronimo, one case at Guichicori and one case at Tapana.

State of Chiapas: One case at the port of Tonalá.

State of Colima: One case at the port of Manzanillo.

The total number of cases noted on the Gulf of Mexico was 893, and the total number of those noted on the Pacific ocean coast was twenty-seven. Grand total, 920.

As will be seen from what has just been stated, yellow fever has appeared in nine towns of the State of Veracruz, in two towns of the State of Tabasco, in one town in the State of Tamaulipas, in one town in the State of Yucatan, in another town in the State of Campeache, in five towns in the State of Oaxaca, in one town in the State of Chiapas, and in one town in the State of Colima.

The figures relating to the cases noted in the above towns, show that the only place in which it might be said that there has been a real epidemic, is the port of Veracruz, in which the distribution of cases and deaths during the year counting from the month of October, 1901, to September of 1902, is as follows:

Number of Cases.		Deaths.
1901.		
October	68	17
November	95	29
December	41	25
1902.		
January	14	2
February	7	6
March	18	10
April	34	18
May	110	46
June	98	42
July	58	36
August	103	35
September	75	29
Total cases	721	274, that is to say, 40%

To the above data I desire to add the following: The epidemic at Veracruz has been diminishing rapidly. As soon as the Supreme Board of Health accepted the doctrine that the *Stegomyia fasciata* is the transmitter of yellow fever, and with the proper permission of the local authorities of the places invaded by the disease, it has put in practice the regulations contained in a circular that has been sent to all the authorities of the Republic calling attention to said doctrine, and advising the sanitary measures which should be put in practice in order to isolate the patient and for the destruction of the mosquito. With this object in view, the government of the State of Veracruz, amended its sanitary code, in order to carry out said measures, and immediately organized some brigades under the direction of Dr. N. del Rio, delegate from the board, and said brigades were directed to exterminate the larvæ of the *Stegomyia fasciata*. These brigades have been constantly operating ever since and if the results have not been entirely satisfactory, it is owing to the fact which I shall presently explain. Indeed, during the present year the work of sanitation of the city of Veracruz has been carried on and a new sewerage system has been built and the laying of the main pipe which shall distribute the potable water has been commenced. This work which has been done with more or less irregularity from a hygienic point of view, has resulted, during the rainy season, which continues through the months of July, August, September and October, a time in which the temperature reaches its highest degree, in the formation of large pools where water accumulated and became stagnate, and where mosquitoes of all kinds were reproduced. Therefore, although all the deposits of potable water in the houses were inspected and examined, and in spite of the fact that a considerable quantity of petroleum was emptied and sprinkled on the pools, formed in the street, as these latter were indefinitely multiplied on account of the rains and of the sanitary work of the port, the work of said brigades to destroy the mosquito has been as successful as could possibly be expected.

Subsequently the rainy season having ceased, and as the drainage work progressed, the extermination of the *Stegomyia fasciata* has been more successful. When the last member of the American commission which went to study the yellow fever at the port of Veracruz, left said port, it had became more difficult to find the larvæ of said mosquito, and the epidemic was gradually diminishing, until only a few cases were noted during the last week of the month of November.

Owing to the importance of the matter, I will report in advance the fact of a yellow fever epidemic which appeared and prevailed during seventy days in the city of Orizaba, in the State of Veracruz; this extraordinary phenomena in our history of the National medical geography repeated itself, for as has been stated in this Association, until the year of 1899, yellow fever had not spread at a greater height than 1000 meters above the level of the sea. In view of the new theory, the *Stegomyia fasciata*, concerning the transmission of yellow fever, we can now easily explain this epidemic as well as the previous one. In fact inasmuch as experience has shown that once the yellow fever has been incubated at Veracruz, just at the level of the sea, said disease might appear at Orizaba in 1200 meters, and since no doubt was entertained as to the possibility of the yellow fever producing agent being able to live at such a height, even though it could not be reproduced there, because until the year of 1899 it had never been noticed that the disease had propagated, when it manifested itself in a person that went up to the above stated height.

To bring this report to an end I would add that the measures relating to the isolation of the patients of yellow fever have been strictly carried out at Orizaba, and that a campaign for the extermination of the *Stegomyia fasciata*, was organized as far as circumstances would allow it. The results obtained have been remarkable, inasmuch as this second epidemic, which broke out with more alarming symptoms than that which appeared three years ago, has lasted a shorter period, and has now ceased entirely.

I have only to add, since other persons more competent than myself will report concerning this matter, that the commission of American and Mexican physicians who studied yellow fever this year at Veracruz, confirms the experiments by Reed, Carroll and Agramonte at Havana, have obtained a positive and exceedingly remarkable result, by inoculating an immune person by means of the *Stegomyia fasciata* which had been infected by a yellow fever patient.

As regards the short epidemic of yellow fever which prevailed at Tampico, as a consequence of the three cases before mentioned, our delegate to said port explains the same in the same manner as explained by Dr. del Rio, without having any knowledge of the hypothesis of said doctor, and to which I have just referred. From what has been set forth in the latter part of this report we may arrive at the following conclusions:

1. It has been confirmed that the only medium of transmission of yellow fever heretofore known, is the bite of the *Stegomyia fasciata*, when previously infected.

2. That the only means to prevent the propagation of said disease are (*a*) the isolation of the patients in such a manner that it will be impossible for the mosquito of the kind specified to bite them; (*b*) the disinfection for the purpose of destroying said mosquito and (*c*) the destruction of the larvæ of said insects by the means recommended in the aforesaid regulations.

3. That it is essential to study the origin of the new epidemics which may appear in any locality in the light of the new theory of transmission of yellow fever, by the *Stegomyia fasciata*.

REPORTS ON THE ORIZABA (MEXICO) YELLOW FEVER EPIDEMICS.

By Dr. NARCISO del RIO, Veracruz, Mex.

In compliance with the order sent by telegraph by the President of the Supreme Board of Health, I proceeded to Orizaba for the purpose of studying a disease that had appeared there, in order to determine the character thereof. Immediately after my arrival I went to see the mayor, who informed me that there had occurred twelve cases, ten of which had been fatal, and there still remained a woman at the hospital and another convalescent patient at his house, who was in his eighth day of illness.

Accompanied by Dr. Labardini, a member of the municipal board of health, I went to the hospital, where I saw the woman mentioned by the mayor, who appeared to be about 40 years of age. At that time she had been sick four days, and her illness was an acute case of yellow fever. Then I endeavored to determine whether she had contracted the disease at Orizaba or whether she had been in some place where the disease was prevailing, with the result that she had not been out of that town. This patient occupied a room at the hospital where she was isolated from the rest of the institution, without any wire gauze at the doors and windows and her bed did not have any canopy. The special department for the patients of this disease was then in process of construction. The day after my arrival, I went with Dr. Ernesto Arzamendi to visit two patients who were being treated by him in the central part of the city, a fact which aroused his suspicion, because all the cases noted had appeared in one single district, namely, that of the Veracruz station, which was far away from the center of the city. These two patients belong to one family, namely, the husband and his wife, the former being at that time in his sixth day of illness and seriously ill, because he could no longer pass any water and uremia had already set in. The illness of the woman was not so serious, and she was in her fourth day of illness, that is to say, when fever commences to gradually diminish. Afterward, I went with Dr. Duplan to visit a girl who was being attended to by Dr. Moya, and who, at that time, was in her third day of declared yellow fever. As this was the most recent case that had appeared there, and in order to better determine the nature of the disease, although clinically speaking I did not entertain any doubt of it being a case of yellow fever, I took some blood from this girl, kept it in a suitable holder, surrounded it with sufficient vaseline, and took all necessary precaution in order to send it to Veracruz, so that the American and Mexican commission might make the proper microscopic examination. I took said blood at 2 o'clock in the afternoon, and six hours later it was examined by Drs. Matienzo, Parker, Rosenau and Beyer, who ratified the complete absence of the "Laveran hermatozoario," and therefore the malaria nature of the disease was excluded, and proved thereby the established clinical diagnosis. Aided by this data, I endeavored to investigate myself how the disease had commenced, which had now assumed the nature of a small epidemic, limited to a small area, almost a block of houses, near the Veracruz railroad station. I thought, of course, I would find a good basis for my investigations, bearing in mind the fact that yellow fever prevails epidemically at Veracruz and that some cases had been noted at Coatzacoalcos, from which latter place several patients had arrived at Cordoba by the line from the Pacific to Veracruz, there being a possibility that in case they should arrive at said city, they might reach as far as Orizaba. Therefore, there are two dangerous places for Orizaba at the present time, namely, Veracruz and Coatzacoalcos, the more so, because the communication by railroad is short and very frequent. The first case known was that of an unknown man who entered the hospital in a dying condition, where he died a few moments after arrival. After the autopsy of the body was made the symptoms of the disease, which had caused the death of the patient, being very suspicious, all the characteristics, traces and features of the yellow fever were found, but it was not known where the patient had got the disease, nor the place whence he came, for as I have already stated, nobody knew him. Perhaps this case is the origin of the other cases which followed, but the medium of transmission from that first case to the other cases has not, so far, been found.

It having been proved beyond any doubt whatever, both by the experiments made at Havana, and by the last experiments made by the United States and Mexican Commission at Veracruz, in order to study yellow fever, that the *Stegomyia fasciata* is the medium of the transmission of yellow fever, as announced many years ago by the learned Dr. Finlay, of Cuba, it was necessary to investigate whether in Orizaba existed mosquitoes of that kind, or if the immunity that until recently was thought to have existed there as regards yellow fever was owing to the fact that said insects cannot live there or reproduce themselves at the height at which said city is located. For that purpose I visited the houses where there had been some cases of the disease, accompanied by Dr. Labardini, first, and afterwards accompanied by the sanitary agents appointed by the chief of police. In all said houses I found larvæ of the *Stegomyia fasciata*, and two mosquitoes of this kind entirely developed. These two mosquitoes furnished me the explanation of the two cases which were noted in the central part of the city, quite distinct from the original focus, namely, the married couple, to which I have already referred, and which were attended by Dr. Arzamendi, and concerning which I secured the following data: The husband was a coachman who had charge of carrying to the hospital in his carriage a sick woman who lived in the infected district, and whom he had to carry in his arms because her condition was so serious that she could not walk. The room which said patient had occupied was disinfected by means of sprinkling bichloride of mercury, and it was in said room that I found the specimen of the *Stegomyia fasciata* entirely developed, it being very probable that the coachman was bitten there by one of the infected mosquitoes, thus transmitting the disease to his wife. I would further add, that in the room in which the couple dwelled, Dr. Arzamendi and myself saw several mosquitoes on the wall, although we could not catch them in order to determine the species to which they belonged.

Therefore, we have now two explanations equally acceptable, namely, either the first case came from Veracruz, Cordoba or Coatzacoalcos, and from said case were infected the mosquitoes found at Orizaba, thereby propagating the epidemic, or the mosquitoes already infected at Veracruz were carried by rail to Orizaba, where the first patient contracted the disease from the mosquitoes which arrived in the manner already described.

The last theory is most acceptable and it is to be wondered at that the disease did not appear more frequently, owing to the facility of communication. In the last epidemic as in the present one, the disease commenced and remained almost limited to the district where the Veracruz railroad station is situated. At that time the origin of the disease was traced to wastes from the Montezuma brewery, which is situated on the opposite sidewalk of said station, and as said brewery has a great deal of communication with the port, and has closed special cars in which beer and ice for the consumption of Veracruz is conveyed, it is quite possible, that while said cars remained at the port some mosquitoes, already infected, were confined therein, and in opening the car in Orizaba, in order to unload it the mosquitoes might get out and station themselves in the neighborhood or bite some of the passengers that are continuously to be found in that direction, either of those from the railroad or from the brewery.

This can account for the fact that it was once said that the cause of the propagation of the disease was the waste from the brewery, not because of said waste itself, but because in the water thereof were stationed and reproduced mosquitoes which were the conductors of the disease.

It seems to me that this latter circumstance deserves to be taken into consideration by the board, since it is a real danger to which all the towns situated on the line of the railroads which touch at Veracruz, and are in the neighborhood of Orizaba are exposed. I think that said danger could be easily avoided by simply requiring that every closed car leaving Veracruz be disinfected before starting therefrom. In order to carry out this operation, the only object of which as will be readily understood, is to kill the mosquitoes that may be found or exist within the car, there should be installed at each station a depot for burning sulphur, which depot should be provided with an exhaust pipe, in order that the vapors may be carried to the interior of the car which it is desired to disinfect through an opening made in the place thought to be most convenient, and said opening should be closed by means of a plug, just after the vehicle is filled with sulphurous vapors.

The Pullman car should be subjected to a similar operation, employing for that purpose the pyrethrum vapors in order not to injure the decorations of the car.

In my opinion the other passenger cars do not need to be disinfected because of the fact that they are open and the air currents pass freely through them, and hence it would be difficult for the mosquitoes to shelter themselves therein.

I believe the method I have just pointed out would not only give practical results, but would be an economic one and would not cause delay to the trains, and for this reason I have the honor to submit it to the board, in order that if it should meet its approbation, it be adopted in the form, and at the time which may be deemed most convenient.

Since, as I have already stated, the disease is limited to a small area, I think it would be easy to exterminate it in a short time, if the measures recommended in the pamphlet entitled "Instructions," published by said board, be established with the efficiency and rigor necessary, thereby organizing brigades for the destruction of the mosquitoes, their eggs and larvæ by means of petroleum, as well as by isolating the patients, so as to render it difficult or impossible for the insects to be infected by them, and thus serve as a vehicle for the propagation of yellow fever. In this case the disinfection by means of bichloride should be substituted for the sulphurous vapors. The above suggestions I thought proper to make to the authorities of Orizaba, and I have no doubt that they will adopt them, since both the welfare and the progress of said city require it.

THE DISINFECTION OF RAILROAD CARS AS A PRECAUTIONARY MEASURE AGAINST THE PROPAGATION OF YELLOW FEVER BY MOSQUITOES.

By DR. MANUEL S. IGLESIAS, Veracruz, Mex.

When publicity was given to the first labors of Reed, Carroll and Agramonte, reviving and supporting the theory that Finlay propounded respecting the transmission of yellow fever by means of mosquitoes, the evidence did not appear to us conclusive, because their work had not been subjected to the strict scientific investigations that were necessary to carry conviction, and after a careful study we came to the following conclusion: "In short; we do not absolutely deny that the mosquito may exercise a certain influence in the propagation of yellow fever, as it is possible that a study may prove this a fact; but neither can we admit this theory, as is done by those who, after a superficial study of the question, proclaim it as an unquestionable truth and one that is scientifically established. We merely suspend our opinion in the hope that new investigations under more favorable conditions and supported by a multitude of experiments will bring us to a certain knowledge of the subject, and at the same time lamenting that the haste with which this conclusion has been reached has given rise to the criticisms that we present in this work."[1]

From that time to the present date, the experiments have been repeated by different investigators and in different localities, and although they are open to the same objections as those which we then criticised, the results obtained have been so conclusive and identical, the experiments have been so repeated and numerous, that we cannot do less than give way before the evidence of the facts and accept the resolution of the Second Pan-American Medical Congress, which met in the city of Havana on the 2d of February of this year, and which was as follows: "The propagation of yellow fever by the *Stegomyia fasciatus* is the only means demonstrated up to the present date for transmitting the disease;" and we have accepted this resolution in spite of the fact that our reasoning points out that the experiments have not complied with all the requisites of a scientific investigation that would place them above criticism.

Allowing that the mosquito above referred to is the only proved means for propagating the disease, but at the same time bearing in mind that there may be others, as yet unknown, but still efficient, it becomes our duty to wage war on that pernicious insect with all the activity and energy in our power, because although we do not clearly know the other means by which this disease is propagated, there is no doubt that by suppressing the one we do know, we will obtain an appreciable diminution of its ravages.

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[1] Gaceta Medica de Mexico, Vol. I, 2d Series, 1901, page 157.

It is not our intention to enter at present on the methods of destruction that ought to be adopted in the cities in which the insect is found, but rather to study the methods to be employed in order to avoid its transportation by railroad trains, as in this way it is very possible that yellow fever may be carried from place to place by such a traveler.

Both by observation and by reasoning we can easily understand that different kinds of insects are transported in the cars, together with the goods which they carry, and even in the passenger cars, and amongst these insects are the mosquitoes. This fact is well known to all travelers by railroad, and there is no reason to doubt that amongst these insects there is probably a greater or smaller number of infecting mosquitoes, which not only bite the passengers, but also scatter themselves in the air of the stations and propagate the disease in other places where it is least expected. This theory appears to be confirmed by the following observation: The disease that presented itself within a very limited area this year, in the city of Orizaba, was diagnosed as yellow fever by the majority of the physicians who had an opportunity to observe it, whilst only a very few opposed that diagnosis. Dr. del Rio, the second delegate of the Supreme Board of Health in the port of Veracruz, who was commissioned to make a study of the case, is of opinion, in view of the chronological observations of the cases that presented themselves and of the data acquired with respect to the influence and action of the mosquito as propagating agent of the disease, that the origin and spread of the epidemic took place as follows:

The first case was observed in the wife of a railroad employe, who up to that time had enjoyed good health and had not come into contact with diseased persons of any kind, contaminated objects or dejections of yellow fever patients, and in a word, presented no appreciable motive for the attack. This woman died, and after her body was taken out of the room in which she died, it was closed and not reopened for several days, when a man was put in to whitewash it and twenty days after he was also attacked. Three other persons were attacked almost at the same time in other rooms of the same house, and thus the first focus of infection was established. After a careful analysis of the facts, Dr. del Rio came to the following conclusion: That an infected mosquito came in the train from Veracruz to Orizaba, and on being released when the car was opened in the station, it remained there or in the neighborhood and bit this woman, who was accustomed to go to the station twice every day in order to carry meals to her husband, who, as before said, was employed there. It is probable that for the moment the woman was unfortunately in a specially receptive condition, and so fell a victim to the contagion. The mosquitoes which subsequently bit her were also infected, and it is to be noted that although we have found an abundance of *Stegomyia* in Orizaba, they never infected other persons until twelve days after the woman was attacked, to which we must add the period of incubation of the disease, and for these reasons fresh cases only appeared twenty days after the first.

Those who obstinately refuse to believe in the intervention of the mosquito in the propagation of the disease, entirely scout this explanation and even ridicule the idea; but although the fact is improbable, it is by no means impossible, as we know that mosquitoes have been found in the ordinary passenger coaches as well as in the Pullman sleeping cars, and if they find a lodging in coaches which are more or less open, there can be no doubt that they are also to be found in freight cars, and therefore in passing from one place to the other there is risk of their carrying the infection, if they find themselves in an atmosphere that is adapted to the development of the disease.

However unlikely this method of transmission may appear, it is our duty to prevent it, and still more so, seeing that up to this date we have employed a multitude of preventive measures that were founded on conjectures, on theories and of opinions that were difficult to demonstrate, for want of positive knowledge of the means by which the disease is propagated. Now that one of them has been demonstrated, we would be greatly to blame if we did not adopt the means we know, especially as they are of such a simple and easy character.

Of all the methods at our disposal for the destruction of these insects, we will only study those that are most efficacious and easiest to put into practice, and which we here enumerate in the order of their activity: Sulphurous acid, tobacco smoke and pyrethrum powder.

Although the first of these is the most efficacious and most rapid in its action, as a stay of a few hours in an atmosphere that is saturated by this acid is sufficient to kill all the mosquitoes without fail, it could not be used except in freight cars, both because these can be kept hermetically closed, as well as because, on account of its action on colored goods, if employed in passenger or Pullman cars, it would seriously damage all the hangings and upholstering of these cars, and this forms a great obstacle to its employment.

The smoke produced by the combustion of the tobacco is highly poisonous to mosquitoes, and we can prove this by introducing a small quantity into a test tube which contains some of these insects, and we will see them immediately die. It is therefore only necessary to produce a sufficient quantity of smoke to dislodge the air contained in the space that we desire to clear of mosquitoes, for them to immediately die, and as for this purpose we would only employ the stalks of the leaves, which are very cheap, they can be burned in any desired quantity.

These two methods that can be adapted to closed cars which are intended for the transportation of goods, can not be employed in passenger or Pullman cars; the first because it takes the color out of all objects with which it comes in contact, and the latter because it leaves everything impregnated with a very strong and disagreeable odor of nicotine; but for these cars we could make use of the pyrethrum powder.

This powder is obtained by the drying and pulverizing of the inflorescence of pyrethrum parthenium which contains a volatile oil with a strong and penetrating odor that is avoided by all kinds of insects, but is not injurious or disagreeable to man in the small quantities that it would be used for this purpose. If these powders are placed in the interior of wardrobes, bookstands, etc., the small amount of oil that it can in this form discharge is sufficient to drive away moths, beetles and other insects, and it would produce the same effect with greater ease if burned, as it would thus discharge its oil more rapidly. It is this powder that, employed in the small pastiles which are commonly sold, drives the mosquitoes out of comparatively large rooms, and it would only be necessary to burn two or three of these pastiles in each passenger coach after closing it to drive out the mosquitoes that might penetrate into it and kill those that could not get out, without any danger of any others entering when the coaches are open a few moments before the passengers arrive, as the effects of these powders make themselves felt for some time after they are burned.

With the methods above indicated, we would obtain the complete disinfection of the railroad cars and their freedom from yellow fever germs in a much more complete manner than formerly without causing any injury or trouble to the railroad companies and with a great saving of time which is an important consideration as regards traffic. All these advantages would greatly facilitate the task of protecting towns and villages from the invasion of this disease a matter of much more importance than the Finlay theory which is already confirmed by the studies and experiments of Reed, Carroll and Agramonte, besides being also corroborated by the investigators who have subsequently studied the matter with such great advantages to humanity in general.

AN OFFICIAL EXAMINATION OF THE WATERS OF THE UNITED STATES.

By MARSHALL O. LEIGHTON,

Hydrographer, United States Geological Survey.

The student of economy generally finds that society appreciates the advantages which have been won by hard labor to a greater degree than those which have been freely bestowed by nature. Whenever we investigate an abundant natural resource, we are almost certain to find that it has been neglected, unwisely developed or wantonly abused, and he who ventures to forewarn the public of loss or famine as a result of such procedure, must be ready to receive the disapproval of his fellowmen. There are indeed a few cases in which natural wealth has received considerate treatment, and as a result, they have become famous without advertising.

Among the free and abundant resources of nature, few, if any, are of so direct and immediate value as water. It is a source of wealth undetermined in its magnitude. Throughout the world's history the location of water has determined the courses of migration, established the prosperity of colonies, located the sites of great cities, and enriched the agricultural, manufacturing and commercial worlds.

With these introductory statements, I wish to introduce to the members of this Association a work recently inaugurated by the Division of Hydrography in the United States Geological Survey. This division has, as its principal duty, the determination of the natural water resources of the country, and during the past years, has issued reports containing run-off data computed from weir or current meter measurements in the important streams of the United States.

Power sites have been surveyed, favorable situations for reservoirs have been exploited, and as fast as possible, an inventory has been made of available water resources. Now it is well understood that in determining the value of any commodity, the damages visited upon it must be taken into account as faithfully as the *prima faciae* value which it possesses. If, therefore, the spirit of the act of Congress directing the Geological Survey to determine water resources be carried out, an estimate must be made of the damage done to our rivers.

Water is applicable for use in many of the lines of special development only when it is in its normal condition, that is, when it is not contaminated with the refuse which results from the occupation of the surrounding country by man. Therefore, when pollution is introduced into a stream, there is an immediate decline in the value of water, that is, the resources vested therein begin to fail. The usefulness of the stream as a source of water supply is reduced; the harvests of ice which may be gathered from its surface becomes dangerous, for, although the efficiency of ice as a means of disease dissemination has, by recent research, been called in question, the idea of serving such a product in our foods and upon our tables is abhorrent and will not be tolerated. Water full of chemical refuse is fatal to boilers and manufactured products are ruined by it. Fish are driven away or killed by pollution. Foul odors arising from a sewage polluted stream depreciate realty values, and in some cases power values are damaged, especially when the filth held back by mill dams becomes so objectionable as to reduce the price of the surrounding real estate by an amount greater than the value of the power site.

It will be observed that the considerations here advanced are based solely upon the theory that pollution is a damage to vested resources. We shall find that the reasons which make stream pollution undesirable are mainly economical ones. There is a broad distinction which must be made between a series of water examinations made for public health purposes, and for the purpose of determining natural wealth and the damage visited upon it.

The hygienic significance of river pollution has grown quite narrow. In former years, when we conceived that the plague and pestilence lurked in fetid vapors and foul odors, a polluted stream was logically a thing for sanitarians to conjure with. With the rise and demonstration of the germ theory of disease, our beliefs have been changed and we have found by progressive steps that our health is less and less in danger from foul water, until it is now generally conceded that in order that a person may be injured by it, he must actually pour it into his mouth and swallow it. Therefore the only feature or consequence of stream pollution which remains of professional interest to the sanitarian, is that of the protection of waters used for domestic supply and the epidemics of this caused by impure drinking water. Therefore it seems that by far the most fruitful, most logical approach to the question of stream pollution is by the path of the engineer and economist, rather than by that of the sanitarian, although the latter retains premises of great importance. There is a feature in the consideration of epidemics of water-borne diseases which must be considered separate and apart from the purely humanitarian aspect; each life sacrificed through the agency of polluted drinking water is worth a certain amount of money, the amount varying according to the age and productive capacity of the individual. The value of each life so sacrificed must be added to the bill of damage accruing against water pollution.

For years we have been preaching the gospel of sanitation to our legislators and our makers of history. We have upheld humanitarian principles, we have pointed to the bodies of the dead, have echoed the wails of the suffering, and we have been laughed at for our pains. If you will glance about the world, you will see wonderful improvements in all matters pertaining to the physical health and comfort of the human race; but how much, think you, would have been brought about if health and comfort alone had been considered? Do you know of a city in this broad land that has installed a system of water filtration because of the suffering of its inhabitants? Those with which I have been familiar have been constructed because the poor water and high death rate resulted in a bad reputation abroad, in decreased business, and depreciations of the value of real estate, therefore, in the struggle against stream pollution, it may be wise to avoid the subject of health and sanitation and meet this mercenary state of society with a mercenary argument. It may be wise to talk dollars and cents even if we are reduced to the consideration of the pecuniary value of lives lost in an epidemic.

These are the facts which appear to the officials of the Division of Hydrography, and the investigation of them throughout the United States becomes the duty of that division. One by one, commencing with those most important, the various drainage areas of the country are being investigated. In each, the character of the normal water is to be determined and the applicability in the different lines of water utilization will be considered. Then by taking into account the flow of the stream, its grade and the country through which it runs, we shall be able to determine how valuable is the drainage system to its area. If we start at the head of the area and determine by analysis and physical examination the effects of the different points of pollution, we can secure a comparative view of the condition of any drainage district, and can readily show that water pollution is one of the costly abuses that has arisen through municipal indecency.

The chemical analyses necessary in carrying on this work are not being made at Washington. Distance from the greater part of the field would make it impracticable to do so, even if at the capital there were proper equipment and a sufficient number of analysts not otherwise engaged. We are establishing co-operative systems with every chemical laboratory that can be secured. It will be in effect, a campaign of encouragement, the results of which are to increase the interest of the chemists throughout the country in the importance of water analysis. It will also serve to bring to the attention of a large number of persons the effects of water pollution. Certain problems are assigned to the director of each laboratory signifying his willingness to enter into the arrangement, and, in the courses of instruction, in thesis work, and by special arrangement, we can secure the data necessary for our purposes. Many of the leading colleges and universities have entered into the work with gratifying enthusiasm.

In order to secure uniform results, we have adopted the procedure set forth in circulars 8 and 9, of the Division of Hydrography. In selecting methods it has been a matter of pleasure to us to adopt those recommended in the progress reports of the committee appointed by the Section of Bacteriology and Chemistry to determine upon standard methods in water analysis. We propose to follow the advice of that committee, for we believe it to be a remarkably able one. The Geological Survey will thus be instrumental in exploiting one of the most important and useful purposes of this society, that is, the bringing to a uniform and rational basis the tangled methods used in water analysis.

Having briefly defined the scope of the system which we have put in motion and explained the basis upon which it rests, let us glance briefly at the problems which are encountered. We find a country of widely varying topography, divided into areas called states, each with a local government. These states are separated from each other, in the majority of cases, by lines which, except from a purely legal standpoint, are not divisions at all. The prosperity of one state does not stop at the state line, it normally extends across; unfavorable conditions on one side of the fence are acutely felt on the other side. Rivers flow across this line or trace out the line itself. The damages which occur in that part of the area included in one state are traceable below within the boundary of the next, that is, there are always interstate features which cannot be controlled by either of the members. It appears as if a national authority over such matters would be advisable.

Looking broadly at the river drainage areas, we find them divided into two approximate parts; first, the headwaters, usually unpolluted; second, the lowland valleys. Highland regions, usually unproductive, are the proper regions for storing water; it should be the broad state and national policy to set them apart forever for forest production and water conservance. The lowland valleys are always polluted when inhabitated, and such a condition is inevitable. The natural drainage of occupied land must be contaminated and the drainage done thereby must be considered as a part of the price which society has to pay for its development. Aside from this pollution, which arises from natural drainage of occupied land, we find two other broad classes—municipal sewage and industrial wastes.

We will pass over the consideration of the first, for we have dwelt upon it so long in meetings of this character and have been so successful in providing means for its purification that the interest therein has well nigh lost its novelty.

The great pollution problem of today is that of the purification and utilization of manufacturing wastes. There is excellent ground for the belief that there should be no such thing as industrial waste and the time is coming when the existence of such will be considered an evidence of poor business management. In many cases the wastes of a few years ago are now utilized at a profit greater than that obtained from the original manufactured product. We must, nevertheless, recognize that industrial wastes exist. What are you going to do with an industry that pours thousands of gallons of polluting material into a stream? There is the industrial plant, retaining certain rights and possessing a standing before the law. It is a purchaser of raw products, an employer of citizens and a distributor of necessary or desirable commodities, the manufacture of which results in the creation of this waste. It seems that the most promising course to be followed is that of taking into account the possibilities of recovery of valuable materials, and the study of this subject will be more efficacious than any other preliminary method of procedure which has been defined. The industrial plant possesses riparian rights, and the liquid wastes which they discharge cannot accumulate and must run down hill. The only method seems, therefore, to be that of purification, and in such cases purification should mean utilization. The opportunity afforded the American Public Health Association along this line is extremely valuable.

I have thus briefly outlined the extensive work undertaken by the Geological Survey. As you have already seen, its success depends largely upon the support given by the scientific bodies throughout the country. To set in motion a great system of water examination embracing many laboratories and many sections of this broad land is not an easy task and we recognize that it will be attended by many imperfections and unfortunate experiences. At the same time the work will from the very start retain much that will be of value and the tendency of the movement will be constantly in the right direction.

We wish to enlist the co-operation and support of this society. There is no reason why the relations between the Geological Survey and the American Public Health Association should not be of the most intimate. There is, to be sure, a broad difference in the paths by which they are approaching the question of water pollution; this Association holds hygienic considerations uppermost, while the Geological Survey has no official interest in that phase of the subject. The former body is actuated by motives entirely humanitarian, while the latter considers mainly the natural resources; a man killed through the agency of polluted water is from the point of view of this Association a victim of criminal circumstances; to us he represents so much loss in dollars and cents to be added to the cost of pollution. At the same time the results which we secure for the accomplishment of our purpose will be equally useful when interpreted from a hygienic point of view. It will be encouraging if the members of this Association will feel a personal interest in the matter, will give their assistance, counsel and criticism, and feel at liberty to make suggestions at any time. The country is broad and each locality has its peculiar problems, but if the hearty coöperation which has already been manifest continues and grows normally, we may be able to accomplish much that will be of advantage.

THE WATER SUPPLY OF NEW ORLEANS AND ITS IMPROVEMENT.

By ROBERT SPURR WESTON, Boston, Mass.

When one visits Louisiana, particularly New Orleans, for the first time, his attention is forcibly turned toward the rain water cisterns which are attached to nearly every building. Many of these cisterns are two-storied. (This construction permits the supplying of the upper floors during times of drought, as the rain water usually first runs into the upper cisterns and then overflows into the lower.) The large rainfall is utilized for the domestic supply, because of the poor quality of the ground water and the hardness and muddiness of the river water—the two other available sources. About nine-tenths of the city's domestic supply is from the cisterns, and the remainder from the Mississippi River, the latter also being used for protection against fires, for flushing gutters, etc. The quality of the rain water varies from very good to very bad, depending upon the location and nature of the collecting surface, the construction of the cistern, the character of the surrounding atmosphere, the season of the year, etc., as the following analyses will show:

TYPICAL ANALYSES OF NEW ORLEANS RAIN WATERS.

(Parts of Million.)

Source of Sample.			Cistern, Girls' High School.	Old Cistern, 1004 Aline Street.	New Cistern, 6028 Garfield Street.	Old Cistern, 718 Dumaine Street.
Date of collection			May 5, 1901.	Aug. 30, 1901.	Dec. 26, 1901.	Dec. 26, 1901.
Temperature,—Degrees C.			.....	.....	8.5	10.0
Odor,	Hot		.....	.....	0.	0.
	Cold		0.	0.	0.	1. Moldy
Turbidity,—Parts silica			0.	1.	0.	1.
Color,—Hazen's standard			5.	3.	5.	12.
Oxygen consumed			0.70	2.50	1.05	1.10
Nitrogen as	Albuminoid Ammonia	Total	0.030	0.075	0.031	0.070
		Suspended	0.000	.....	0.000	.....
		Dissolved	0.030	.....	0.031	.....
	Free ammonia		0.005	0.020	0.020	0.490
	Nitrites		0.000	0.000	0.000	0.005
	Nitrates		0.09	0.12	0.080	0.000
Chlorine			.....	4.05	4.20	4.05
Alkalinity			.....	3.8	.....	.....
Residue on Evaporation	Dissolved		.....	23.	12.	35.
	Suspended		.....	1.	0.	1.
	Total		.....	24.	12.	36.
Iron			.....	0.30	0.10	0.75

Naturally, the well constructed and well cared for cisterns collecting water from clean roofs in the more open parts of the city, and automatically wasting the first portions of each shower before turning the rain water into the cisterns, may be expected to furnish satisfactory water. On the other hand, the seldom cleaned, open cisterns receiving the washings of befouled, dusty and decaying shingle roofs in the thickly settled districts, are undoubtedly frequent sources of infection.

However, two conditions militate against the use of even the most excellent cistern, namely: The long, dry season and the increased use of water due to the increasing amount of sanitary plumbing. Again, the construction of the proposed sewerage system will invite the construction of more house plumbing and will consequently demand more water than the sixty inches of rainfall can conveniently supply. Already many of the better class of houses use the rain water for cooking, drinking and laundry purposes only, while the river water is used at the sinks, lavatories, baths and water closets. Still other houses are provided with small filters, some of which filter river water to supply the whole house, thereby doing away with the questionable cistern water and its unsightly container.

The necessity of soon obtaining a more abundant and satisfactory water supply for New Orleans has been realized for some time, and a sewerage and water board was constituted by a legislative act of 1899, to build new water supply and new sewerage systems. To defray the costs of these works, a 2-mill tax was levied, which permits a bond issue of about twelve and one-half million dollars.

There exists here the unusual example of an American city without sanitary sewers and with only about one-tenth of the houses connected with the existing mains of the local water company.

Three sources of water supply were proposed, namely: The rivers to the north of Lake Pontchartrain, the ground water from local deep wells and the Mississippi River.

The water of the rivers north of the lake is generally uncontaminated by sewage, but would certainly require storage or filtration before use, to perfect its appearance and protect it against chance contamination. This purification, and more especially the cost of a long conduit, pumping machinery, etc.—exceeding the available funds—prohibited the adoption of this source of supply.

The water of the deep wells, while excellent for use in steam boilers, is alkaline and has such a high color that purification would be necessary were it used for domestic purposes. This purification would be expensive. Furthermore, the possibility of assuring a supply from this source is doubtful, and was enough in itself to prevent the serious consideration of this source of supply.

There remained only the Mississippi River—abundant, near at hand and contaminated only at remote points, but on the other hand, moderately hard and laden with suspended silt and clay.

Previous attempts to purify the river water made by the New Orleans Water Company, in 1884, using mechanical filters with coagulant, but without preliminary subsidence of the raw water, had resulted in a costly failure, and therefore grave doubts were expressed regarding the practicability of any system of water purification, especially upon a municipal scale. However, the Sewerage and Water Board was advised by its experts that if the river water were subjected to proper preliminary treatment, it could be filtered afterward without serious difficulty.

Upon the advice of Mr. George G. Earl, M. Am. Soc. C. E.,—its chief engineer—the board decided to conduct an investigation as to the feasibility of purifying the Mississippi River water, under the general supervision of Mr. George W. Fuller, Assoc. M. Am. Soc. C. E., the consulting sanitary expert of the board, and Mr. Earl, and under the immediate charge of the speaker.

WATER PURIFICATION INVESTIGATION.

A water purification station was established at Audubon Park, and, during the flood season of 1900-01, the behavior of the Mississippi River water was observed under various systems of treatment and after various periods of subsidence, with and without the aid of a coagulant.

This station contained four complete and adjustable systems of water purification, having a total daily capacity of 93,000 gallons per diem, or enough for 1500 people. Each system consisted of subsiding basins and filter. The basins could be operated either with or without the aid of a coagulant, and the four filters, two slow and two rapid, were supplied with the effluents from one or another of the basins. Thus was it possible to study, among other things, the effect of plain subsidence for various periods, the effect of supplementary subsidence with a coagulant, the efficiency of the various methods of filtration, the effect of various sized sands, the effect of various rates of filtration, etc., and to compare the data one with another. The station was equipped with an adequate laboratory and the work of the station was performed by a staff of four trained and four untrained assistants, besides the speaker. The cost of the investigation was something over $24,000, an expense which will undoubtedly effect a saving of several times that amount, besides furnishing valuable initial data for the operation of the plant in practice.

The details of the investigation, including the results of several thousand analyses are given in a recent report to the Sewerage and Water Board, by the speaker.

Among other things, the investigation determined the character of the Mississippi River water, the most efficient and economical method of filtration, and besides, furnished important data for estimating the cost of the proposed systems of purification.

COMPOSITION AND CHARACTER OF THE MISSISSIPPI RIVER WATER.

The Mississippi River is a clay-bearing stream, whose water at New Orleans possesses the following characteristic features:

1. Wide variation in the amount of suspended matter. This variation is not so marked, however, as in many of its tributaries.

2. The comparatively large proportion of the suspended matter, which is made up of fine clay particles.

3. The absence of sudden changes in the amounts of suspended matter in the water.

4. The frequent but not sudden changes in the character of the water, due to the predominance of one or another of the tributaries.

5. The absence of appreciable evidences of sewage contamination. This is because of the great dilution of the sewage entering the stream, the remoteness of the sources of pollution, and the almost complete purification effected in the river itself by natural agencies, during the stream's passage through the delta, the surface of which drains away from, instead of into, the river.

It may be well at this point to give a brief description of the watershed, which of course determines the character of the river water at New Orleans.

One will recall that the watershed of the Mississippi has an area of about 1,240,000 square miles, distributed among thirty-one states and territories, and its diversity of character may be readily comprehended when one considers that within the Mississippi River basin are the slopes of the Alleghanies and the Rockies; the lakes of New York, Minnesota and the west; the limestone farming country of Kentucky and Tennessee, and the black bottoms of the Dakotas; the arid plains of the central west and Colorado, and the swamps of the central valley.

The Mississippi basin is made up of six tributary basins, namely, the Ohio, Upper Mississippi, Missouri, Arkansas, Red and Central Valley.

One is aware of the fact that because of the wide variations in amount of rainfall in different parts of the basin, and because of the varying character of the soil and climate, the discharge of the tributaries is not uniformly proportioned to the areas of their several basins. The following table shows the relative stream discharges, areas and rainfalls for the various tributary basins, as compared with one another:

Basin.	Stream Discharge.	Area.	Rainfall.
Ohio	1	2	2
Upper Mississippi	4	4	4
Missouri	3	1	6
Arkansas	5	3	5
Red	6	5	3
Central Valley	2	6	1

This table illustrates the well-known fact that the discharge of the Missouri River is much less than the discharge of the Ohio, although its volume of water area is much greater.

The amount of rainfall varies from 0 to over 60 inches, and averages about 29.8 inches per annum, according to the best authorities.[1] This rainfall results in a stream discharge of from 200,000 to 2,000,000 cubic feet per second, or in other words, from 0.12 to 1.2 cubic miles per day.

As has been stated previously, the contamination of the river by sewage is not at all serious in comparison with that of most other American streams. One is so accustomed to hear the Mississippi River called the "main sewer of the continent," "natural carrier of the waste of a teeming civilization," etc., that he is surprised to find that it is a difficult matter to demonstrate, by the most delicate tests, that the local river has been contaminated. This is evidenced by the fact that *bacillus coli communis* was discovered only once or twice in five months, although 300 c. c. portions of the water were taken for each determination, and that *bacillus enteriditis sporogenes* (Klein) which is readily isolated from the Red River water at Shreveport, was found at New Orleans on but one occasion only.

The urban population of the valley is estimated at 9,000,000 people, including all towns which have a population of 4,000 or more, as determined by the United States census of 1900.

Only four cities with a combined population of 43,961, discharge their sewage into the river within 600 miles of New Orleans, while the levied banks prevent the accession of surface drainage.

As you well know, the land of the delta is necessarily highest at the banks of the stream, thus preventing surface drainage. It is evident that the dilution of the contamination is enormous, and that much opportunity is given for the self-purification of the stream by natural agencies, while it flows on its way to the Gulf.

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[1] Report of U. S. Weather Bureau. Report of Mississippi River Commission, 1900.

The Mississippi River is a very muddy stream, containing on an average about 2.7 tons of suspended matter per million gallons of water. This means that any system of purification must remove enough silt and clay from the future water supply of New Orleans to cover four city squares three feet in depth per annum.

The economical clarification of the water by the removal of the clay and other suspended matter is therefore indirectly the chief end of any system of water purification at New Orleans, and experience during the investigation confirms the opinion that this removal of clay is accompanied by an adequate removal of bacteria and color, thereby guaranteeing protection against the germs of disease, which might perhaps, at some time, be present in the unfiltered water.

Many thousand analyses have been made to determine the composition of the river water, with the result shown in the following summary:

SUMMARY OF AMOUNT OF CONSTITUENTS FOUND IN THE RIVER WATER FOR THE PERIOD BETWEEN DEC. 10, 1900, AND AUG. 17, 1901.

	Parts Per Million.		
Constituents.	Maximum.	Minimum.	Average.
Silica turbidity	1460.	90.	405.
Total suspended matter	1040.	75.	440.
Total dissolved residue	250.	80.	145.
Suspended albuminoid ammonia	0.598	0.015	0.188
Nitrogen as total albuminoid ammonia	0.677	0.054	0.251
Nitrogen as free ammonia	0.036	0.000	0.012
Nitrogen as nitrites	0.023	0.000	0.008
Nitrogen as nitrates	0.56	0.02	0.14
Chlorine	20.9	6.0	9.2
Incrusting constituents	24.	5.	14.
Alkalinity	115.	57.	79.
Dissolved oxygen	11.5	5.3	9.0
Free carbon dioxide	75.	0.	34.
Bacteria per cubic centimeter	6500.	60.	2065.
Temperature, degrees C., (degrees F.)	31.1 (88.)	7.0 (44.6)	17.6 (63.8)

From data collected during the investigation and obtained from the Mississippi River Commission, it was possible to determine closely the physical character of the river water, as is shown in the following summary:

SUMMARY OF THE PHYSICAL DATA.

Average suspended matter, 1900-1901	441	parts per million
Average silica turbidity, 1900-1901	406	parts per million
Average turbidity coefficient, 1900-1901	1.08	
Average recorded suspended matter, 15 years	[1]688	parts per million
Average suspended matter, corrected for analytical errors	650	parts
Average turbidity coefficient for 22 samples of water having between 600 and 700, and averaging 648 parts of suspended matter per million	1.07	
Estimated mean silica turbidity	600	parts
Maximum suspended matter, 1900-1901	1040	"
Mean maximum suspended matter, 15 years	[1]2500	"
Estimated mean annual maximum silica turbidity	1500	"
Estimated mean annual minimum silica turbidity	125	"

METHODS OF PURIFICATION.

Three general steps in the systems for complete clarification and purification have been considered in various combinations, as follows:

1. Plain subsidence in basins for several days.

2. Supplementary subsidence in basins with the aid of a coagulant.

3. Filtration, either at a slow rate through sand beds—English filters—or at a rapid rate through filters provided with mechanical devices for cleaning the sand—American filters.

1. *Plain Subsidence. Relative Fineness of the Suspended Matter.*

The suspended silt and clay in the Mississippi River water at New Orleans contains much very fine material, material immeasurably finer than the smallest bacteria. This is probably because of the opportunity afforded for sedimentation—of the larger particles—during ordinary stages of the river, on the bed of the river itself. These larger particles then become broken into or worn to sizes which can be carried down stream by the lower velocities which obtain in the last few hundred miles of the river.

An idea of how fine these particles are may be obtained by comparing the approximate percentage of suspended matter removed from the local water for twenty-four hours of plain subsidence, with that removed from the muddy waters of other localities for the same period of subsidence, as follows:

Locality.	River.	Estimated percentage removal of suspended matter in 24 hours.
Kansas City	Missouri	82.
Cincinnati	Ohio	62.
New Orleans	Mississippi	45.

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[1] Report of Mississippi River Commission.

PLAIN SUBSIDENCE INADEQUATE TO PREPARE WATER FOR FILTRATION AT ALL TIMES.

On account of the large amount of fine clay particles, the suspended matter, as a rule, is held in suspension indefinitely and cannot be removed, after any practical periods of plain subsidence—even periods of a week or more.

ENGLISH FILTERS AND PLAIN SUBSIDENCE.

When this subsided water is filtered at slow rates through thick layers of fine sand, a satisfactory clarification and purification results for but a portion of the time. This system would produce muddy effluents for months at a time, and—what is more serious—the cost of cleaning and renewing the clogged sand layer would be prohibitive.

2. *Supplementary Subsidence with the Aid of a Coagulant. Necessity of Coagulant.*

Since the fine particles of suspended matter cannot be removed by plain subsidence, it is necessary to add something to the water to bring them together in aggregates or groups, which of themselves would have a subsiding value great enough to cause them to settle within a reasonable period. Sulphate of alumina can be used for this purpose and when added to the water forms a perfectly insoluble gelatinous precipitate which, uniting with the clay, causes it to gather into flocks or aggregates which settle much more rapidly than the diffused clay particles themselves. This precipitate also has the power of attracting, enveloping, or absorbing stray particles of suspended matter including bacteria, so that the water by this method of coagulation and supplementary subsidence can be made fairly clear.

DIFFICULTY OF REMOVING LAST TRACES OF TURBIDITY BY COAGULANT.

In practice, water which has been purified by plain subsidence followed by supplementary subsidence with a coagulant is, strictly speaking, never perfectly clear. This is because it is difficult to remove the last traces of turbidity by this process alone. Indeed, it is much more satisfactory to make the final step in the clarification and purification process by the use of a suitable filter.

3. *Method of Filtration.*

Two kinds of filters can be used for this final clarification and purification of the effluent of the coagulating basins, namely, the English, or slow, and the American, or rapid filters. These filters are well known to all. The English system, you will recall, consists of a bed of sand through which the water, without coagulation, but after being subjected to plain subsidence, filters at a slow rate, while the American system treats the water after both plain subsidence and also supplementary subsidence with the aid of a coagulant, and filters it at a rapid rate through a bed of sand which is intermittently freed from matter which obstructs the passage of water by reversed currents of water and by agitation. In the English filter the accumulated matter which clogs the filter is removed by draining and scraping.

The main features of the two filters, however, are compared as follows:

	English Filters.	American Filters.
Construction	Covered.	Partially covered.
Area of units	About 1 acre.	15 x 24 feet.
Area of 40 million gallon plant	10 acres.	0.32 acre.
Net yield or rate of filtration, million gallons per acre per 24 hours	4	125
Depth of sand—inches	36.	30.
Depth of gravel—inches	12.	4.
Underdrain system	Drain tiles and conduits.	Pipe system of double bottom and metal strainers.
Clogged sand layer cleaned by	Draining and scraping.	Washing with filtered water until cleaned.
Vertical height of structure	14 feet.	15 feet.
Relative efficiency for clay removal, and relative bacterial efficiency	Satisfactory.	Satisfactory.

BOTH FILTERS APPLICABLE.

Both filters would be applicable to the purification of the Mississippi River water, provided suitable arrangements for plain subsidence and supplementary subsidence with the aid of a coagulant were provided:

The sizes and arrangements of the two systems of purification may be compared as follows:

	Modified English System.	American System.
Total daily capacity of plant—gallons	40,000,000	40,000,000
Capacity of plain subsidence basin in hours flow	12	12
Capacity of coagulating basin in hours flow	24	12
Average amount of coagulant—grains per gallon	3.7	4.5
Coagulant used	Practically continuously.	Continuously.
Approximate first cost of plant	$1,260,000	$700,000
Total estimated cost of purification per million gallons, including interest, depreciation and operating expenses, as well as pumping water from river to suction of high lift pumps	$16.46	$15

It is seen that the American system is less expensive to construct for use in New Orleans than the modified English system by more than $500,000.

SELECTION OF SYSTEM BEST ADAPTED TO LOCAL CONDITIONS.

Either filter would be adapted to the purification of the Mississippi River water and the decision as to which would best suit local conditions should be dependent upon the cost per million gallons of filtered water, taking into consideration the difference in first cost.

In total first cost, the American system is fully 25 per cent. cheaper than the modified English system, when estimates are made on the same basis.

CAREFUL SUPERVISION NECESSARY WITH BOTH SYSTEMS.

Both systems require careful supervision and attention, as is obvious.

FINAL CONCLUSIONS.

In conclusion, it may be said that the present water supply of New Orleans is neither satisfactory nor abundant, and that sufficient data has been collected during the investigation to allow a system of water purification to be designed which will efficiently and economically purify the Mississippi River at New Orleans. Furthermore, the available evidence shows that the American system is best adapted to local conditions on account of its low first cost, coupled with adequate efficiency.

During the investigation frequent changes were made in the sand used in the rapid filters, and the bacterial efficiency suffered accordingly. Since the investigation, however, one of the units has been operated for the purpose of supplying water to the wading pool in Audubon Park. Although this plant has been operated during part of each day only, it has shown a bacterial efficiency of 98.2 per cent. This is certainly in excess of the requirements. Furthermore, the appearance and taste of the purified water leave little to be desired.

ON THE RELATIVE IMPORTANCE OF PUBLIC WATER SUPPLIES AND OTHER FACTORS IN THE CAUSATION OF TYPHOID FEVER.

By W. T. SEDGWICK and C.-E. A. WINSLOW, Massachusetts Institute of Technology, Boston.

The rôle of public water supplies as vehicles of typhoid fever was early made apparent by the epidemic at Lausen, Switzerland, in 1872, and that at Caterham, England, in 1879. In this country evidence of a similar character was not long lacking; for in 1885 the thriving mining town of Plymouth, Pa., suffered one of the most disastrous water epidemics of which we have any record. The great epidemics at Lowell and Lawrence in 1890 added new emphasis to the old lessons; and only two years ago over 450 cases of typhoid fever at New Haven, due to a combination of circumstances nearly parallel with those of the Plymouth disaster, showed that the teaching of experience has not yet been thoroughly mastered by all communities.

Even more serious, in the long run, than these sharp epidemic outbreaks has been the steady incidence of typhoid fever upon communities having water supplies constantly polluted to a less extreme extent. Reference to the instructive table published by Dr. S. W. Abbott in his review of "Public Hygiene and State Medicine in the United States" will make this fact very clear. While most of the cities for which returns are tabulated have typhoid death rates ordinarily ranging from 10 to 30, there are five, Albany, Jersey City, Philadelphia, Pittsburg and Washington, which are distinguished by figures running up to 50, 75 and 100 deaths per 100,000 of the population. In the last three cities taken together nearly eight thousand persons died of typhoid fever during the nine years—1890-98. With all these facts in view, it is no wonder that the latest monograph on typhoid fever[1] states that "among the carriers of virus, water is, according to the present state of knowledge, by far the most important." The same opinion from the standpoint of the engineer was voiced by Mr. M. N. Baker[2] as follows: "Not only is typhoid one of the leading causes of death in America, but the greater part of it is conveyed, directly or indirectly, through water."

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[1] H. Curschman, Typhoid Fever and Typhus Fever, American edition, edited by W. Osler. W. B. Saunders & Co., Philadelphia, 1901.

[2] M. N. Baker, Municipal Engineering and Sanitation. Macmillan, New York, 1902.

Fortunately the progress of popular sanitary education is effecting a material change in this respect and the replacement of unguarded neighborhood wells and polluted rivers by naturally or artificially filtered supplies or by large storage reservoirs has become general in the more thickly settled regions of the country. It is well therefore to recognize the fact that the conditions which previously obtained, and under which water was by far the most important vehicle of typhoid fever, belong in many communities rather to the past than to the present. Jersey City, for example, where the typhoid death-rate ranged from 52 to 94 between 1890 and 1896, abandoned in the latter year the use of the Passaic river at a point where it was heavily polluted by the sewage of Paterson and Passaic, with the result that the rates for the four years immediately succeeding fell to 21, 36, 15 and 21. Albany effected an equally striking reduction in its typhoid death rate when a great municipal filter was introduced in 1899; and the other communities mentioned above, Philadelphia, Pittsburg and Washington are taking measures to secure similar immunity. Before many more years the large American city which drinks the unpurified water of a stream, or of a lake with unprotected watershed, will be an anomaly.

Of special interest in this connection are the statistics of the State of Massachusetts, where the supervision of the public water supplies has perhaps been more thoroughly carried out than in any other American commonwealth. In the period 1886-90 the typhoid death rate for Boston was 39 per 100,000 and for Newton with an admirable ground-water supply was 40. The rate for all the cities of the state taken collectively was somewhat higher, 46, and the two large cities, Lowell and Lawrence, drinking the polluted water of the Merrimac, had each a rate of 112. As later events showed, two-thirds of the typhoid fever in Lowell and Lawrence was caused by water and this excess of course materially raised the rate for the total urban population of the state.

Between 1891 and 1895 the unpurified river-water supplies of Lowell and Lawrence were abandoned, and for 1896-1900 their rates fell to 25 and 24. In at least one of these communities, Lowell, the present water supply, from a series of driven wells, is absolutely protected from infection. In Newton with a rate of 20 and in Woburn with a rate of 13, the conditions are similar. Gloucester, Pittsfield and North Adams, with rates of 16, 25 and 39 respectively, possess surface supplies of unexceptionable quality from uninhabited watersheds, as free from pollution as any such waters could possibly be.

The North Adams typhoid rate is higher than that of any other city in the state and may be regarded as due to some exceptional conditions. We may, however, consider the other five communities as typical of most of the cities of the state, differing only in the fact that our knowledge of the purity of their water supplies is more certain than in other cases. In these five cities we find typhoid rates of 13, 16, 20, 25 and 25. We are confident that these rates are not raised by pollution of the public water supplies and that the influence of private well water upon them is inappreciable. It therefore follows that typhoid deaths ranging from 15 to 25 per 100,000 are due in these cities to causes other than polluted water; and it is probably fair to assume that in similar American communities about this amount of typhoid fever may be expected to occur without its intervention. When we find that the typhoid rate for the whole of Massachusetts in 1896-1900 was only 24, and in 1901 only 19, it seems improbable that in this State water is "by far the most important" vehicle of typhoid fever.

The difference between the typhoid due to public water supplies and that which occurs in cities whose water supply is unpolluted is often shown quite clearly by the seasonal distribution of the disease. In communities of the latter sort the typhoid, or as it was called by the older sanitarians, the "fall fever," follows the curve of temperature with extraordinary regularity. If the monthly deaths from the disease be plotted and compared with the monthly temperatures it will be found that the curves are almost parallel, the typhoid fever rising with the temperature after about two months; an interval representing the incubation period of the disease and the time which elapses before death. The diagrams for New York, San Francisco and Santiago (Fig. 1) show typical curves of this sort, in which the typhoid tracing has been shifted back two months so that the course of the lines may be easily compared, the March typhoid deaths thus corresponding with the January temperature. It will be noticed that where the temperature curve is acute that of the mortality follows it, whereas in a mild climate the distribution of the disease is more even. In the southern hemisphere both curves are reversed. We have elsewhere shown that these phenomena are so constant and so universal as to suggest in the strongest manner a direct relation between temperature and typhoid fever; and our theory of this relation is as follows:[1]

"The bacteriology and the etiology of typhoid fever both indicate that its causal agents cannot be abundant in the environment during the colder seasons of the year. The germs of the disease are

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[1] Statistical studies of the seasonal prevalence of typhoid fever in various countries and its relation to seasonal temperature. By William T. Sedgwick, Ph. D., and Charles-Edward A. Winslow, S. M., in Memoirs of the American Academy of Arts and Sciences, Vol. XII, No. V, August, 1902.

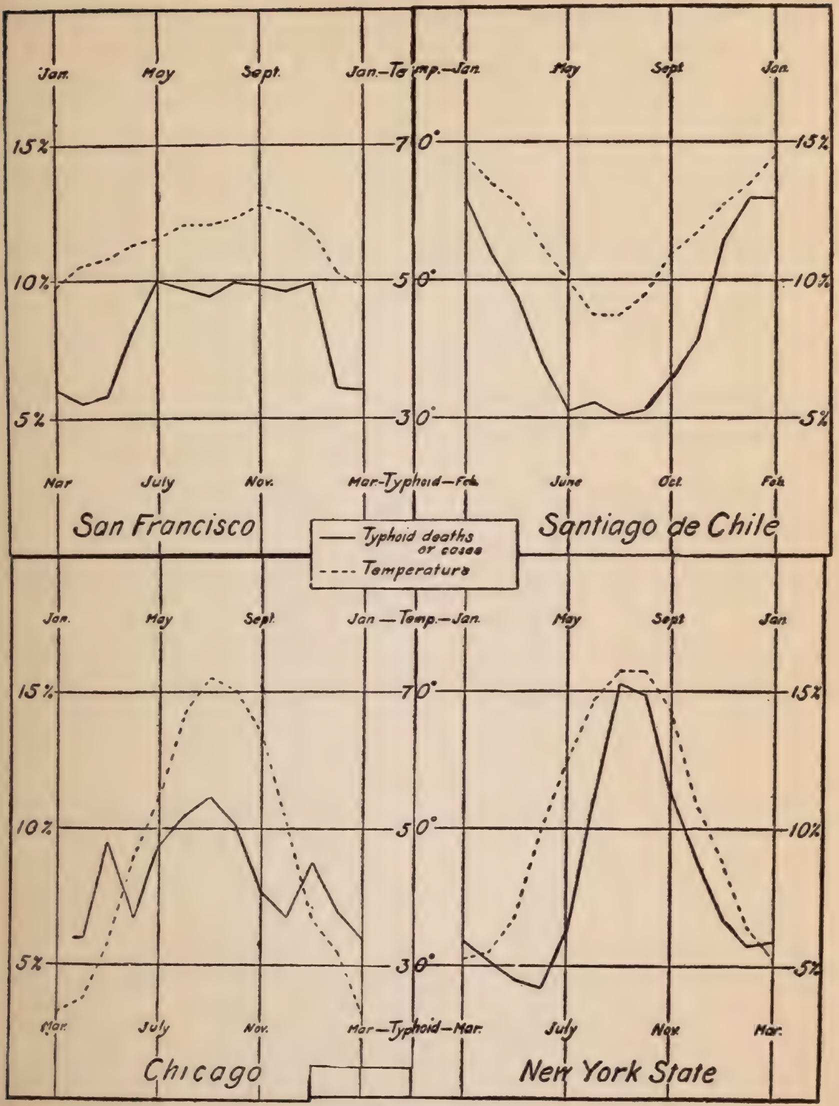
Jan.
May
Sept.
Jan.—Temp.—Jan.
May
Sept
Jan.
15%
70°
15%
10%
50°
10%
5%
30°
5%
Mar
July
Nov.
Mar-Typhoid—Feb.
June
Oct.
Feb.
San Francisco
Santiago de Chile
—— Typhoid deaths or cases
---- Temperature
Jan.
May
Sept.
Jan —Temp.—Jan.
May
Sept
Jan
15%
70°
15%
10%
50°
10%
5%
30°
5%
Mar.
July
Nov.
Mar—Typhoid—Mar.
July
Nov.
Mar.
Chicago
New York State

FIGURE 1.

carried over the winter in the bodies of a few patients and perhaps in vaults or other deposits of organic matter where they are protected from the severity of the season. The number of persons who receive infection from the discharge of these winter cases, will depend, other things being equal, upon the length of time for which the bacteria cast in the discharges into the environment, remain alive and virulent. The length of the period during which the microbes live will depend largely upon the general temperature; as the season grows milder, more and more of each crop of germs sent at random into the outer world will survive long enough to gain entry into a human being and bear fruit. The process will be cumulative. Each case will cause more secondary cases; and each of the latter will have a still more extensive opportunity for widespread damage. In our opinion the most reasonable explanation of the seasonal variations of typhoid fever is a direct effect of temperature upon the persistence in nature of germs which proceed from previous victims of the disease."

In cities having a polluted water supply there is an interesting departure from the normal seasonal distribution of typhoid. We find the normal maximum in September or October; but in some cases there are also secondary maxima in the fall and spring as shown in the curves for Chicago (Fig. 1). These high death rates in the cooler months were at first puzzling, and such instances have obscured the usual seasonal variations and led some sanitarians to deny the existence of any relation between typhoid fever and temperature. As soon, however, as it appeared that curves of this type were associated with polluted water supplies their significance became clear. The fall and spring epidemics coincide with the fall rains and the spring thaws, which wash infective material from vaults on the bank, or through storm overflows, into the water supplies, carrying it fresh and virulent to the consumers below. A seasonable distribution of this sort appears to be characteristic of many communities drinking from unprotected surface waters; and it was found by us in this country in Chicago, Cincinnati, Newark and Philadelphia. On the other hand the large majority of cities, including Atlanta, Baltimore, Boston, Charleston, Denver, Mobile, Montreal, New Orleans, New York, St. Paul, San Francisco and the District of Columbia, showed a close and constant relation between the temperature and the seasonal prevalence of typhoid fever.

With regard to the prevention of water-borne typhoid little needs to be added to the accepted theory; and common practice, at least in the larger cities, is rapidly nearing a satisfactory state. It becomes, however, more and more necessary that the attention of sanitarians should be directed to that tax of 15 to 25 deaths per hundred thousand population which typhoid fever levies on communities provided with ever so pure a water supply. To what shall we attribute that minimum of this disease so constantly present in most American cities? Partly, no doubt to occasional epidemics in which the vehicle is some material other than water; to small milk epidemics, shell-fish epidemics, celery epidemics, etc. Outbreaks of any size in which the infection of a large number of persons can be traced to a common source are, however, rare. Those recorded as due to milk, although considerable, account for but an insignificant proportion of the total and constant annual mortality; and epidemics traced to shell-fish and raw vegetables have been still more exceptional. As for ice we have elsewhere shown by experimental evidence that this probably plays an inappreciable part as a vehicle of typhoid fever.[1] Most of the typhoid cases which occur cannot be connected by any common epidemiological bond, but follow each other in a slow succession of sporadic cases or in that closer association as to locality to which the older sanitarians applied the term "endemic." As this word is outworn, because by custom associated with the idea of some mysterious but wholly intangible quality inherent in a definite soil or region, the term "prosodemic,"[2] has been suggested by one of us, for this form of disease "which progresses gradually from person to person by routes which, whether direct or indirect, are often different for each individual case," the term epidemic being restricted to "that special case in which circumstances permit the transfer of infection to a large number of persons through the same medium, and at approximately the same time."[3]

The insidious course of typhoid fever spread in prosodemic fashion has never been shown more clearly than by Dr. William Budd in his classic monograph published in 1873. The little village of North Tawton in England suffered a severe outbreak of this character in 1839, over eighty cases occurring between June and November of that year, following each other slowly, several members of a family taking the disease in almost every house in which it appeared. Three infected persons left the place during this summer and each one proved a new focus for the spread of fever in a region otherwise free from it.

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[1] Experiments upon the effect of freezing and other low temperatures upon the viability of the bacillus of typhoid fever, with considerations regarding ice as a vehicle of infectious diseases. By William T. Sedgwick, Ph. D., and Charles-Edward A. Winslow, S. M., in Memoirs of the American Academy of Arts and Sciences, Vol. XII, No. V, August, 1902.

[2] Gr., *pros*, forth, from (one point) toward (another), toward, before, in presence of, hard by, near, etc., and *demos* the people.

[3] Typhoid fever at Newport, R. I., in 1900, and its relation to defective sanitation. By Charles-Edward A. Winslow, S. M., Technology Quarterly, Vol. XIV, No. 2, June, 1901.

In the first case the two children of the sufferer succumbed to the infection; in the second case a friend who acted as nurse took the disease and in turn transmitted it to his two children and his brother. The third of the visitors left North Tawton to stay with a brother at another hamlet seven miles away. The brother's wife, the brother himself, two farm hands, a friend who had come to take charge of the house, another farm hand, two servants and the daughter of the original visitor, successively developed typhoid fever. Evidence of the direct transmission of the disease could scarcely be more conclusive.

In this country the outbreak at Bondsville, Mass., where one hundred cases occurred among the mill operatives in the summer of 1892 (see Massachusetts State Board of Health Report for that year) and that at Newport, R. I., in 1900 in which some forty cases, apparently caused by an infected well, were followed by double that number of secondary cases, furnish more recent examples of the same phenomena. In each of these instances the slow succession of cases and the bond of physical association between the victims gave evidence of the prosodemic character of the disease.

The methods by which prosodemic typhoid may spread are almost infinite in number. The chain begins with excreta and ends usually with food; but the intermediate links may be few or many in number. The fingers of unprofessional attendants upon typhoid patients or of the patients themselves during the early stages of the disease, and flies, which so often pass freely from infected privies to adjacent larders, suggest themselves as ready vehicles. Sometimes the route is so direct that typhoid fever, under uncleanly conditions, becomes for all practical purposes a contagious disease. Sometimes it is so roundabout as to baffle any attempt to trace it. In all cases the proper treatment of excreta is the only certain preventive measure; and a too common neglect of the old-fashioned panaceas, cleanliness and disinfection, is the sole cause of the excess of prosodemic typhoid.

To recapitulate, we have undertaken to show:

First—That in the State of Massachusetts and in some of the larger cities of the United States the public water supplies are now unimportant as vehicles of typhoid fever.

Second—That in cities having pure water supplies the annual curve of typhoid fever mortality closely follows that of annual temperature.

Third—That in urban communities supplied with pure water there still remains a typhoid fever tax of from 15 to 25 deaths per 100,000 population.

Fourth—That this tax is due not to any peculiar condition of soil, locality or climate (endemic factors), but to incomplete disinfection of typhoid excreta with subsequent infection of various articles of food and drink. These factors, when acting upon a few or many persons at one time, may cause obvious epidemics, sometimes large though generally small; but more often the infection in moving from one point to another follows different and obscure routes for different victims, and hence may be described as *prosodemic*.

Fifth—That the only remedies for such prosodemic typhoid are absolutely thorough and universal cleanliness and disinfection of excreta.

DIRT, DISEASE, AND THE HEALTH OFFICER.

By Dr. CHARLES V. CHAPIN,
Superintendent of Health, Providence, R. I.

When our honored and lamented Reed went to Havana and discovered that yellow fever was transmitted by the bite of a mosquito, and Gorgas, by the most brilliant sanitary experiment ever made, put an end to this disease in its very stronghold, they drove the last nail in the coffin of the filth theory of disease. But it is to be feared that the devotees of this theory are loath to bury it, thus violating one of their cardinal principles. It seems to me that it is the duty of the health officers of this country to see that this ceremony is properly performed. The filth theory erroneously assumed that the infectious diseases were caused by emanations, gaseous or otherwise, from decaying matter. Everything decaying, and everything offensive to the sense of smell was dangerous. Everything dirty, everything nauseous, possibly, nay, probably would cause sickness. It was boldly taught that by removing all decaying matter the infectious diseases could be stamped out. For many years this idea dominated sanitary practice, the communicability of these diseases being almost entirely neglected. The English, who carried this notion of the danger of filth to the extreme, were assumed to be the leaders in public health work, and we blindly followed the leaders. Little stress was laid on *personal* uncleanliness. It was believed that the *municipality* was chiefly responsible for infectious diseases. Pure air, pure water, and a pure soil was the cry. Sanitary reform was engaged principally in protecting drinking water from organic contamination, in building sewers, in developing plumbing to a complicated and expensive art, in cleaning streets, in removing dead animals, in collecting garbage and removing household rubbish, in whitewashing and repairing tenements, in the regulation of offensive trades and the general suppression of all nuisances affecting the sense of smell. Of course there is some truth in the idea that dirt may be the cause of sickness. Some diseases are fecal borne and the danger from this source is sufficient to warrant our treating all fecal matter as suspicious. The protection of water supplies and the construction and utilization of sewers prevents disease and is worth all it costs. The secretions of nose and mouth are so often the carriers of infection that the personal cleanliness which avoids danger from this source is a potent factor in hygiene. But with minor exceptions, municipal cleanliness does little to prevent infection or decrease the death rate. Municipal cleanliness is no panacea. There is no more a royal road to health than to learning. It will make no demonstrable difference in a city's mortality whether its streets are clean or not, whether the garbage is removed promptly or allowed to accumulate, or whether it has a plumbing law.

There is no single procedure by which we can exterminate infection and prolong life. We have learned the true nature of infection and we have learned that the parasites which are its essence rarely propagate in filth and are seldom air-borne. We have, in the language of a distinguished American hygienist, been to a large extent, "barking up the wrong tree." Instead of an indiscriminate attack on dirt we must learn the nature and mode of transmission of each infection, and must discover its most vulnerable point of attack. We have learned that to fight typhoid fever and cholera we must keep human feces out of food and drink; in yellow fever and malaria we must destroy the *Stegomyia* and the *Anopheles;* in smallpox we must vaccinate, and in plague we must kill the rats. It is only along the line of patient investigation of each disease and practical deductions from ascertained facts that public health work can succeed.

There is nothing novel in all this and few will be found to dissent from the above. Rather, I fear, the criticism will be that I am threshing over all straw, and that the filth theory is buried as well as dead. I wish this were so, but I fear it is not, and I am sure that in my own city the outgrown traditions of the middle of the last century are still virile enough to decidedly interfere with real sanitary progress. Some of the most recent works on sanitation still reiterate the time-worn phrase about dirt and disease. The daily press and even the medical press speaks as if street cleaning, scavenging, modern plumbing and tenement house reform were the mainstay in fighting infection and reducing the death rate. The majority of even intelligent people today believe that Havana was made healthy by municipal engineering, while it was really accomplished by scientific effort specifically directed against certain infections. Our medical schools are also doubtless at fault, for many of even our younger physicians look for the source of malaria, typhoid and diphtheria in "unsanitary conditions," by which they usually mean defective plumbing, decaying vegetation, heaps of stable manure, or general yard rubbish. The great problem of sanitation today is how to deal with mild or unrecognized cases of contagious disease and with those persons, who, though well, are yet infected. This problem is not likely to be solved so long as physicians trace infection to the class of *things* mentioned, instead of to *persons*. There is also no question that we health officials, even against our better judgment, cling to discredited methods. An entirely disproportionate amount of time and money is devoted to plumbing regulation and the abatement of minor nuisances which have no direct relation to public health. We have far more sanitary inspectors than medical inspectors, and bacteriologists are not much in evidence except in the largest cities. Popular misconception as to the relation of dirt and disease is illustrated by the advertising cartoons of "Spotless Town," which is supposed to be so clean that the poor old doctor is dying of starvation. But we can rest assured that however spick and span may be the streets, and however the policeman's badge may be polished, that so long as there is found the bore so careless with his expectoration, and the doctor who cannot tell a case of sapolio from one of diphtheria, the latter disease as well as tuberculosis will continue to claim their victims.

Let me not be misunderstood. I am no lover of filth. I believe that personal cleanliness is the most important factor in the prevention of the infectious diseases. I as heartily believe in municipal cleanliness. It is one of the better phases of our modern urban life. The municipality ought to do much more than it ever has done to protect its citizens against nuisances of all kinds. Again, there is no question that municipal cleanliness does some good in the way of directly preventing disease, and also much in an indirect manner, by encouraging personal cleanliness. But there are very many other factors in modern life which tend to promote health far more than the one in question. But the health department, when it interferes with property rights as it does, and expends so much time and money in nuisance abatement, should have some stronger warrant than that it tends indirectly to promote good health.

I would plead with health officers for a more rational perspective in directing their efforts and to devote more attention to the isolation of infectious diseases, medical inspection, disinfection, vaccination and the control of milk supplies, and less to the abatement of nuisances; and in the latter, more attention to those nuisances which clearly and directly menace health, and less attention, or none at all to those which do not. Do not claim more for municipal house cleaning than the facts warrant. Teach, on all occasions, the true relation of dirt to disease. And lastly, pay a little less attention to finical defects in plumbing, to stable manure and garbage buckets, and more to the solution of those problems of infection which are so urgently pressing upon us. To attempt to solve these problems is not so easy as abating nuisances, but it will pay better in the end, and every health officer should do his part in the advancement of sanitary science.

TUBERCULOSIS AND AGRICULTURAL COLONIES.

By Dr. FELIX FORMENTO, New Orleans.

There has been no subject more studied and discussed of late by medical men and scientific bodies than that of tuberculosis. Great progress has been accomplished in every country, in regard to the prophylaxis and care of that disease. We all admit today that tuberculosis is caused by Koch's bacillus, that it is contagious and to a certain point curable—proper hygienic measures and climatic influences being recognized as the most efficient means of limiting the terrible ravages of the disease. The best results have been obtained in properly regulated sanitoria erected in privileged localities. The open-air treatment, carried on, as far as practicable, in all seasons of the year, such as is followed in Colorado and the Adirondacks, has undoubtedly given most satisfactory results. In a great number of cases complete recoveries have been secured.

But it is not sufficient to establish prophylatic measures, to pass ordinances against the disgusting habit of spitting in the cars, or on sidewalks.

It is not sufficient to receive consumptive patients in specially constructed hospitals in the best possible climatic and hygienic conditions. We should do something more to prevent recurrence of the disease; those that have been once affected with tuberculosis and who have been cured, or at least greatly benefited by proper treatment should be kept in the best possible condition in order to prevent reinfection and allow them to earn their daily bread without being exposed to further development of the disease.

What is the condition of the consumptive poor after he leaves the sanitarium? How can he avoid a recurrence, or an aggravation of his disease if he returns to the impure atmosphere of cities, with their crowded and unsanitary lodgings and workshops, "poor food," want of sunlight, of fresh air.

So far very little has been done to secure the continued improvement, or recovery of the patient. I don't know of any after-treatment prophylactic measures adopted in our country. This apparent neglect has induced me to communicate to this learned sanitary association the plan and workings of an institution which came to my notice during my recent visit to France, and which has already given excellent results. I refer to the "agricultural colony for consumptives," established recently at Cannes, in the south of France. Cannes, as you know, is a celebrated resort for invalids. It is one of the prettiest winter stations on the Mediterranean riviera, frequented by the aristocracy of the world; it boasts of having been the preferred sojourn of Queen Victoria, the Prince of Wales and other royal personages.

This colony or home for consumptives was initiated and organized by Dr. Vandremer, of Cannes, some two years ago, and has met with considerable success—more than 100 consumptives have been admitted and continue in restored health. The mayor of Cannes, Mr. Capron, and a general committee composed of its most eminent citizens and foreign visitors, are at the head of it. Dr. Emile Roux, of the Institute Pasteur, is president of the scientific committee which includes several "chefs de service" of said institute.

The main object of the "Colonie Agricole" is as follows: To receive tuberculosis patients when they leave the sanitarium and give them some new trade or occupation, which does not expose them to irritant or contaminated dust, or vapors, to new infections, nor to excessive work or fatigue. They are made to work in the field, in the garden, in the open air, in a delightful climate; they are taught to cultivate the grape or flowers, which are so abundant and varied in that beautiful country. Under these conditions the ex-consumptives soon pick up, they are no longer a burden to their relatives or the community; they receive a certain salary for their work, their moral, as well as physical condition is improved; the mere fact of being independent exerts a most salutary influence.

When they have become experts in their trade, some situation is found for them in that delightful country, in the best climatic conditions, and the working of the sanitarian is thus completed.

Such are, in a few words, the ideas that have inspired the creation of the "agricultural colony" of Cannes.

What has been done on a limited scale in a small city of France could well be imitated and improved in our country. As a rule, the consumptive on leaving the sanitarium, is abandoned to his fate. In the majority of cases he suffers a recurrence of his disease. There is here a big hiatus to fill. What an opportunity for some of our multi-millionaires to illustrate themselves by creating such an institution as that of Cannes and rendering additional service to suffering humanity. We hope and trust that this idea will be taken up and that, under the auspices of this Association, we will, before long, have among us many such "colonies agricoles.

CAUSES AND PREVENTIVES OF INFANT MORTALITY.

By Dr. FRANCISCO SANCHEZ MUNOZ, Mexico, Mex.

Wishing to make this article as practical as possible, I examined the statistics of the civil register of Puebla City, being the only place where I could obtain the facts approximating the truth.

It is astonishing to see the enormous percentage of mortality in the early period of life. In the first year of life one out of every five children dies, and after the age of five only one out of three survives. We know all the difficulties to be encountered in the preservation of health in these young ones. This question of prophylaxis is so important that medicine and hygiene should act as an active and efficacious medium for promulgation. The city of Puebla, at the last census had a population of 100,000, of which 24,000 are children from one to five years of age. During the last five years the mortality was 24,248 persons, of which 10,908 were children from birth to five years old. The financial condition of the city is in direct ratio with the birth and death rate of its inhabitants.

Unfortunately we cannot find the correct cipher denoting the annual increase of population, on account of the many parents who, through neglect or a feeling of apathy do not conform to the law, wherein all births must be reported to the civil registrar.

But, I can truthfully state that the mortality of children in Puebla reaches as high as fifty per cent., which is one-half of the births. The predominant ailments that carry away the majority of our children are as follows: Diseases of the respiratory apparatus, disease of the digestive tract, diseases of the nervous system, general infection, diseases of the circulatory apparatus, and diseases of the genito-urinary tract. The last two named, play not important parts in the death rate, while even less than two-thirds of those who die are from the two first. Among the affections of the respiratory system, broncho-pneumonia is the most frequent. In the digestive disorders may be mentioned gastro-enteritis and enteritis in all its forms. In the nervous system, meningitis and encephalitis are the most important, while in the general infections, we have a comparatively smaller number. The principal ones are, however, smallpox, measles and whooping cough.

Now I will endeavor to bring before you what I think is the most frequent causes of these affections. The new-born infant is very sensative to cold and does not adapt itself to the variations of temperature. A new-born child requires more external heat for its sustenance than any other mammalia, as is confirmed by the mortality during the winter months. Very cold weather may produce death by asphyxia, and any violent change in the temperature is a potent factor in producing various maladies. We must remember the benefits derived by supplying artificial heat to new-born and weak children, as will be seen by the large amount saved by the incubator—thirty per cent. of those born at the sixth month of pregnancy and seventy-eight per cent. of those born at the seventh. In Puebla the temperature never descends very low, but is very changeable. It is necessary to advise, that the little ones should be well clothed and that their cradles be not kept too near the fire. Cold baths should not be given, and in cases in which the calefactory apparatus is wanting the use of cotton and warmth may do some good. It is also our duty to see that the baptismal water used by priests be tepid and not cold.

Digestive innutrition is one of the most frequent causes of death. This condition, however, is generally believed to be due to congenital debility, or to the various forms of enteritis. The digestive organs in the early days of life are in their period of development, and the only food that can be tolerated is milk. There is no doubt that the maternal nutriment is the best, and in France only ninety in every thousand die, of infants nourished by the mother. In our country the women feel proud to suckle their offspring, and only when physically disabled or suffering from disease do they deprive themselves of the pleasure. But among the poorer classes it is customary to give the little ones numerous kinds of purgatives and all kinds of vegetable beverages for the purpose of cleaning the intestinal tract from the meconium. This idea should be refuted by medical men. Where the mother cannot suckle her young one, she should get a wet-nurse, the latter being previously examined by a physician. Allow me to caution you that the child of the wet-nurse should always be examined, as it is the best method of ascertaining the quality of the milk, and in case that the child be dead, investigate the cause. In cases where the family is poor and cannot provide a nurse, the child should be sent to some asylum. The other cause of mortality is due to artificial feeding, and though this method is often serviceable, yet it is inferior to the breast, and only thirty per cent. of the children so fed thrive with it.

In our practice we often meet cases in which artificial feeding must be resorted to. In these cases we should advise the parents on the best methods of conducting it, such as the adulteration of milk, its origin and that it should come from healthy animals, free from tuberculosis and be sterilized by any of the known methods, such as heating, pasteurization, etc. We should endeavor to propagate this rule, as its violation is the cause of many cases of atresia and rickets. This principle of feeding should be kept up until the eighteenth month. The first six months the diet should be exclusively of milk, the second, amylacious foods, and the third, nitrogenous, meat juice and eggs.

The stomach of the child is very small, one hundred grammes at the most is its capacity, so the time of feeding should be at long intervals. During the first month it should be every hour and a half, every two hours the second, increasing little by little until it reaches to be from four to five hours.

After this way of feeding there should be substituted for animal milk, arrow root, tapioca, gluten, etc.

All of these rudimentary rules of hygiene are unknown to our low class of people, who give to the children from their early days all kinds of foods, meat, bread, fruit, and even alcoholic drinks.

This is the cause of such enormous infantile mortality.

Other of the causes is the negligence of the little ones. As the Mexican woman begins to be employed in factories, she is obliged to deprive her children of her care, for necessity of laboring. In Puebla there does not exist any asylum in which the laboring woman can leave her children with the assurance that they would be well fed and cared for.

The nervous diseases seem to be, according to my statistical observations, very common. Notwithstanding this fact, frequently the complications or the symptoms are taken as true disease. Reasons for this are that either the physician is called at the last moment, or there is a mistake in the diagnosis. None of the organs undergo such rapid development as the cerebrum; its weight is duplicated in the first two years. The activity of the nutritive process puts it in condition to contract disease under the influence of multiple and various causes. For the considerable changes of the circulation, as the sutures of the cranium are not ossified, and fontanels are membranous, congestion is very liable to happen. For the same reason they cannot afford to lose a large amount of blood. There also exist sympathetic links with the digestive apparatus, and so we see that its inflammation frequently accompanies the termination of the gastric intestinal diseases. The spinal cord is very excitable. West has said that the spinal system predominates in children over the cerebrum, so in them convulsions take place, instead of delirium.

The infectious diseases have fourth place.

The power of absorption in children is greater, due to the development of their lymphatic system, and delicacy of the epidermis. Notwithstanding this, the absolute lack of hygiene of our people, these diseases do not cause much harm, perhaps due to the benignity of our climate. In fact, the mortality of infectious diseases would be insignificant were it not for the lack of culture and means among our poor class.

Our houses have not the required hygienic conditions, the landlord tries to get all the profit he can, without caring for anything else. The rooms in general are dirty, ill ventilated and they do not have enough water; drainage is very poor or does not exist at all. It is very frequent we find rooms overcrowded. It is from these facts that when an infectious disease occurs in a family it is impossible to enforce separation and disinfection, whose benefits they are incapable of understanding. This is the reason why epidemics break out periodically, as has been proved in the later years by the re-appearance of smallpox. The poor class vaccinate their children because they are compelled to do so, by authority, and always try to avoid this duty. Tuberculosis, though it has been affirmed to be frequent in children, carries off very few of our children.

Erysipelas and post partum hemorrhage may be observed, due to ignorance of the women that act as mid-wives. The poor class cannot afford to pay for the services of a graduated mid-wife.

Having enumerated the principal causes of the infantile mortality I pass to expose the resolutions that I believe would help to diminish it.

First. Promulgation of the hygienic principles. To have a written card specifying the care that should be taken with the new born, insisting upon the value of natural alimentation. Also, rules concerning the hygiene of clothing, habitations and practical means of defence against the propagation of infectious diseases. This card should be delivered by the sanitary inspectors, doctors and midwives, and public lectures should be given them concerning these points.

Second. Children's asylums, where the laboring women could leave their children during the day or when they are sick.

Third. Hospitals and maternity houses, to which every solicitant should be admitted. In Puebla such establishments are private and for a very small number of patients.

Fourth. The Mexican lady is a model of charity. Promote among the ladies the formation of associations with the purpose of giving to the poor mothers both clothing, food and medicine.

Fifth. Investigate to what extent the mortality is influenced by the illegitimacy of children. Such children are generally abandoned by the father, and their education neglected. In Puebla eighty per cent. of the new born are illegitimate.

Sixth. To see that at school and at home a physical education is given.

Seventh. In the five years statistics that I have taken I have found nine hundred and seventy-three children born before complete development. As such a number seems to me very high, and I am sure it is much higher (due to the frequency with which they conceal the cases), it would be of great value to investigate the cases and find a way to avoid them.

WET NURSES—THEIR HYGIENIC IMPORTANCE.

By Dr. RAMON N. PRADO, Mexico, Mex.

Both varied and numerous are the problems that from olden times have occupied the attention of hygienists for the preservation of health and life, and we can easily understand that as infantile pathology contributes greatly to the rate of mortality, principally on account of the gastro-intestinal diseases that are caused through bad nourishment, it is of the greatest importance to establish rules that founded on scientific principles, will be of easy application in practice.

Judging from what we read in the books on pathology and hygiene, the problem of nourishing new-born children would appear to be solved, but we can not say that it is so in practice, in view of the difficulties and special circumstances that obstruct that solution. There is nothing easier or more in accordance with physiology and hygiene than to say that the mother is in duty bound to nourish her children; but it is a matter of common observation amongst practical physicians that it is very frequently impossible to realize this ideal. An excessive organic debility, acute or chronic diseases, whether contagious or not, smallness of the nipple, cracks in the same, phlegmon of the breast, bad quality of milk, etc., are very frequent circumstances that render maternal lactancy impossible. It is also very easy to say that in default of the mother, it is necessary to have recourse to a nurse; but I would ask if this does not present some difficulties. This is the point to which I desire to call your attention.

Without denying for a moment that the proper nourishment for a new-born child is human milk, I can not do less than form this reflection with regard to wet nurses. However small may be the wages paid to these women (which amongst us is not generally very low), is it always within the reach of the poorer classes, or even of the middle class? And when we come to deal with persons in a good position, is it always possible to find a proper wet nurse at the moment of urgent necessity? Let us see. All physicians are aware of the physical and moral qualities that are required in a wet nurse. She must be married, and if possible handsome, young, healthy, stout, and with well developed breasts whose lacteal secretion will be of a good quality. She must be of good character and habits, jovial and merry, without being impressionable, and sober. These are the principal qualities that must be found in a wet nurse before she can merit our approval. Well, now, is it easy, as a general rule, to find wet nurses who combine these qualities? Let the answer be given by the Mexican mothers, for whom it is almost a misfortune to find themselves obliged to search for or tolerate wet nurses, and who consider themselves happy when they find one that is passable. In fact, even setting aside for a moment the physical qualities that we certainly must take into account, let us consider the airs that are put on by these women when they get into a family, their caprices and despotic manners, not only with the other servants, but also with the parents of the children they nurse! How many disputes do they not often give rise to among the servants, and what prudence does not the mother require to exercise in order to settle these quarrels? And what do these mothers not suffer when the nurses demand a further supply of exciting liquors with their food than those that are hygienically expedient;[1] or when it is necessary to keep a watch over their chastity; when they are unclean or when they ill-treat the children under their care?

But there is more yet: In many cases it is not easy, without a prolonged observation, to decide on the goodness of a nurse. If it is a question of her supply of milk, it is very often the subject of deceit, the custom of allowing the milk to accumulate for some hours in order to swell up the breasts and present an abundant secretion at the moment of inspection, is only too common amongst us. If it is a question of disease, it very often happens that, as the inspection of the wet nurse is made in the house of the parents by the family physician, he omits his inspection of the sexual organs, in which are found seventy per cent. of the reasons for refusing the nurses. Or else the examination of the milk is incomplete for want of the special apparatus for this kind of analysis, and an opinion is formed as to the quality of milk, merely judging from its organoleptic character. Besides this, no physician can ignore the really invincible difficulty that often arises against the discovery in a wet nurse, of such diseases as syphilis, tuberculosis, etc. And there is another point still, that from the social point of view is of great importance, and which consists in the fact that ninety per cent. of the children of wet nurses die for want of proper nourishment and maternal care, and this alone is enough to invite us to search for better means of feeding children who cannot be nursed by their mothers.

The above brief remarks clearly show the great danger that attends a mistaken selection of a wet nurse, and the practical difficulties that are encountered in securing a good selection, as well as how easy it is to fall into error on this point. And to think that, it sometimes happens, though not very frequently, that women are found who, anxious to preserve their beauty or for the sake of their own convenience or of social pleasure, will deliver the fruit of their wombs into mercenary hands, placing it in a condition that favors the development of bad health and even the loss of life! To think, also, that there are plenty of physicians who, without good reason and thought, will advise the employment of wet nurses by their patients! It was with good reason that the laws of Lycurgus, of the Athenians, of the Gauls and of the Germans prohibited the nursing of children by mercenary women; and with good reason did a historian praise the customs of the ancient inhabitants of Anahuac on this point. Torquemada says: "There is one thing that these Indians have, that is highly praiseworthy, and that is that they never allow their children to be nursed by strange women, but oblige those who bear them to give them milk until they reach an age to be weaned, and so far was this custom carried, that the kings would not allow their children (who were very numerous on account of the number of wives they had) to be nursed by others than their mothers, and if for any (just) cause they were not nursed by those who had borne them, a nurse was sought for the child *with great solicitude and care, so that she would have good milk.*

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[1] "Pulque,"which is the alcoholic beverage that is habitually used by our lower classes, is imperiously demanded by most of our wet nurses, whilst in other countries it is wine, beer, whisky, etc.

The simple enunciation of these two factors: the qualities that must be combined in a nurse and the defects that are commonly found in them, induces us to look for better and easier methods of nursing children during infancy.

Amongst us, the use of donkey's or goat's milk is little practised. The former is found very expensive, as much or more so than the nurses, whilst the latter require a constant vigilance on account of their mischievous habits. The alimentary flours, on account of their composition, are not adapted to those small organisms.

We now come to the question of cow's milk. When this nourishment is *sterilized*, it fulfills the conditions of economy, facility of preparation and of digestion, and it is also of a nutritive character. It is true that it is necessary to give the child the quantity of milk that is in proportion to the development of his stomach.[1] The

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[1] It is desirable to bear in mind that according to Roch, the stomach has the following capacity, at the ages given:

Newly born	25	cubic centimetres
1 month	75	" "
2 months	95	" "
3 months	100	" "
4 months	107	" "
5 months	108	" "
6 months	200 to 205	" "

These figures are somewhat different to those given by other authors; but the differences are not very notable and they are useful for the purpose of controlling the nourishment by means of artificial foods.

meals must be sufficiently separated to allow for proper digestion, and the nipples of the bottles must be scrupulously looked after from the point of view of cleanliness and asepsis, as can be easily understood. With these requisites, the artificial nourishment deprives of its rigidity the old hygienic precept that human milk is the exclusive food for new-born children, and easily resolves the difficult problem of their nourishment, whenever for just cause it is impossible for the mother to give it, besides not being within the reach of all classes.

It therefore becomes the duty of all practical physicians, as well as hygienists, to teach mothers the manner in which they can properly sterilize milk, so that they can easily put in practice the counsels here given. But if, through any circumstance, it is not easy to sterilize the milk at home, there is no reason why the parents should not accept that which is furnished by the trade, and which is prepared in accordance with scientific precepts and remains in good condition for some time. It is also well that the public should know that when the milk loses its natural color and gives out a bad odor, it has undergone some alteration and is not fit for use.

If, in this country, some factory would specify on its vessels the date of the preparation, it would render a great service to the public and at the same time increase its own credit.

HYGIENE OF THE SIGHT.

By Dr. AUGUSTIN CHACON, Mexico, Mex.

We must consider, among the most difficult problems of ophthalmology, the correction of errors of refraction, though, at first view, it could not seem so.

We have a proof of the complication of this subject, in the fact that, consulting different conspicuous authors, particularly, if of different countries, we find that they do not always agree in its solution.

Another proof is the great number of procedures devised and of apparatus used to estimate the refraction of the eye.

I will attempt to better explain what I have just said:

The prescription of glasses to persons who need them is a problem that requires also the solution of two other problems; the first consists in the determination, as exactly as possible, of the static refraction of the eye and of the amplitude of accommodation. The second problem is (the refraction of the eye known), to choose the proper lens that improves the vision and, at the same time, keeps it in a good condition and removes the symptoms of asthenopia or any others, depending on error of refraction.

The first part of the problem or, rather the first problem, to estimate the exact refraction of the eye, is difficult to solve, and though there are different procedures, they are not all of equal exactness and they don't give identical results.

However, as this is a physical determination of the refraction, it is not necessary, at most, to be practised by an oculist. It is, only, a subject of applied optics, and an optician possessing all theoretical knowledge and the sufficient practice, can estimate the exact refraction of the eye as well as an oculist.

But, in this manner, we have only solved a part of the problem; very interesting, indeed, indispensable, but not the whole. There remains yet the most arduous, the final, *i. e.* the refraction of the eye known, to prescribe the most suitable lens. The glass with which the patient sees the best is not always the one required. This is the truly clinical part of the subject. To solve this question of the prescription of the glasses, we must bear in mind, among other factors, and besides the static refraction which we have already determined, the convergence, the accommodation, the relation between the convergence and the accommodation or the relative accommodation;

the binocular vision, the necessity that the patient has of adapting his sight to different distances, the glasses he is accustomed to bear, the balance of the extra-ocular muscles, and the conditions of the fundus of the eye.

All this, it is easily understood, can only be estimated by a physician. The problem is a very complex one.

The laws of these essentially vital facts, cannot be mathematically determinated, and should they be fixed as exactly as an astronomer fixes the laws of the course of the earth, in any particular case, we could not accurately measure the amount of action of each factor. On that account, we must confess that the determination of the refraction is, and will always be, empiric, notwithstanding that is the most scientific branch of ophthalmology, and even of medicine.

It can now be easily understood how necessary, in prescribing corrector glasses, is the sagacity of the practitioner, and why different oculists, among the skilful, prescribe to the same patient glasses of different number.

Undoubtedly, as they have all the same ability, and having estimated the refraction by the same procedure, they can measure it with equal or almost equal, exactness; but their opinions differ when they must prescribe the lens to correct the vision, because, as I have already said, this is a clinical question and discrepancies of opinions are frequent on clinical subjects

I would not too much insist on proving that an optician who prescribes glasses for the correction of errors of refraction, as learned as he may be, embraces subjects that are not of his competence.

I do not intend in any manner to lessen the merit of the optician's labor, very important indeed, but we must agree in the fact that the task of the physician and that of the optician should be different.

Both labors complete themselves, and, from this division of the work, we shall obtain its best quality and in every sense the best results.

It is the place of the oculist to prescribe the lenses to correct an error of refraction, and the opticians must confine themselves to executing the prescription, with the same fidelity as the apothecary prepares the prescription from a doctor, without any alteration, as the responsibility is all from the ophthalmologist who made the prescription.

Thus we shall realize, in the best possible manner, this so important part of hygiene, and it would be well that some laws rule the practice of the optician.

Only a physician can prescribe the remedies for a disease, and why in prescribing one of the most difficult remedies to choose (the glasses are but a remedy of the anomalies of refraction), should we confide it to the knowledge of a person without medical instruction?

The sense of sight is the most necessary for the enlightenment of man, and we can say that the culture of a people can be measured by the care they take in the hygiene of the sight.

I am sure that the opinion that I have expressed is also that of the American ophthalmologists, and if I have insisted upon it, it is only because it needs to be brought to public attention.

The details of this subject belong to ophthalmology, which is, as are the other branches of medicine, an auxiliary science of hygiene.

OFFICIAL REPORT OF THE PROCEEDINGS OF THE THIRTIETH ANNUAL MEETING OF THE AMERICAN PUBLIC HEALTH ASSOCIATION.

HELD AT NEW ORLEANS, LOUISIANA, DECEMBER 9, 10, 11 AND 12, 1902.

DECEMBER 9—FIRST DAY—MORNING SESSION.

The Association met in Tulane Hall, and was called to order by the President, Dr. Henry D. Holton, of Brattleboro, Vt., at 10:15 A. M.

Rev. Max Heller then delivered the opening address. (See page 18.)

THE PRESIDENT.—We will now listen to the reports of the Committee of Arrangements.

Dr. C. P. Wilkinson, Chairman of the Local Committee of Arrangements, made the following verbal report: Members of the Association are invited to visit the medical department of the Tulane University. Dr. E. S. Lewis also invites you to visit the Charity Hospital of New Orleans. The presidents of the various clubs have extended you invitations to visit those places, particularly the Pickwick Club and the Boston Club.

Wednesday afternoon, at 1 o'clock, an excursion will be given on the steamboat Lotus Sims to Kenilworth plantation. The steamboat will leave the foot of Canal street promptly at 1 o'clock, and will return to the city about 5:30 P. M. Luncheon will be served on board.

THE PRESIDENT.—We will now have a report from the Executive Committee by the Secretary.

THE SECRETARY.—Mr. President: The Executive Committee has held two meetings since its arrival, and I beg leave to present a list of the names of applicants that have been acted upon, and which the Executive Committee recommends for election to membership. (The list of applicants was read.)

DR. FREDERICK MONTIZAMBERT.—I move that the Secretary be authorized to cast the ballot of the Association in favor of the election of those gentlemen whose names he has read. Seconded and carried.

The Secretary then cast the ballot of the Association, as instructed, and the applicants were declared duly elected.

At this juncture, Dr. Eduardo Licéaga, of Mexico City, Mexico, delivered a brief address on behalf of the Mexican delegation, expressing profound sorrow on account of the absence at this meeting of two distinguished members of the Association, namely, Drs. Albert L. Gihon and Walter Reed.

THE PRESIDENT.—We will now listen to the report of the Secretary.

THE SECRETARY.—Your Secretary has no report to make other than to state that the Transactions of the last meeting have been published in Vol. XXVII.

THE PRESIDENT. You have heard the report of the Secretary, and unless there is some objection, the report will stand approved. Hearing no objection, the report is approved.

THE PRESIDENT.—The next thing on the program is a paper entitled "An Official Examination of the Waters of the United States," by Marshall Leighton, Hydrographer, Washington, D. C. (See page 272.)

On motion of Dr. Felix Formento, of New Orleans, the privileges of the floor were accorded to Dr. Eduardo Garcia and Dr. Eduardo Moore, from the Republic of Chili.

DISCUSSION ON THE PAPER OF MR. LEIGHTON.

DR. G. P. CONN, of Concord, N. H.—I rise to ask that this department at Washington take cognizance of the state laboratories, of which in New England we have several. If I understood the paper correctly, no mention was made whatever of state laboratories. We have a laboratory in New Hampshire which is doing excellent work. Massachusetts has long had a state laboratory, and has expended an immense amount of money for carrying on the same investigations that are being conducted in Washington by the laboratory represented by the writer. I congratulate him and the Association that our national officers will take this matter up and carry it along, and in that way we will have federal rather than state work to be done, because it is necessary to establish such laboratories in almost every state of the Union. It ought not to be limited to any one state.

Dr. W. C. Woodward, of Washington, D. C., read the report of the Committee on "Disposal of Refuse Material," in the absence of the Chairman, Mr. Rudolph Hering, of New York City. (See page 21.)

Dr. Charles V. Chapin, of Providence, R. I.—I desire to say a word or two in regard to my own paper, in addition to the excellent summary presented by Dr. Woodward. I do not think the disposal of garbage by feeding it to swine is ideal. It may be considered ideal by Mr. Hering, but to my mind the ideal method of disposal of garbage has not been devised. Neither do I think it is a method which is permissible in the majority of cities. It cannot be done in most cities in this country. However, I believe that there are a number of cities so situated that their garbage can be fed to swine with as little nuisance as would be produced by any method of garbage disposal. This method is unquestionably an economical one, and certainly it is our duty, if possible, to save money for the prosecution of work in other departments, without injuriously affecting the health of our community or without encouraging the production of nuisances. I have had occasion many times to inspect other methods of garbage disposal, and I have seen the crematories and reduction works in different parts of the country, and some of them are far worse than any hog-pen I have ever seen.

The following papers were referred to the Committee on Publication without being read, inasmuch as they were summarized in the report presented by Dr. Woodward: "The Disposal of Refuse in the City of Buffalo, New York," by Mr. Olin H. Landreth, of Schenectady, New York; "The Disposal of Refuse Materials in Cincinnati, Ohio," by Dr. Byron Stanton, of Cincinnati, Ohio; "The Disposal of the Refuse of New York City," by Dr. George A. Soper, of New York City; "The Collection and Disposal of Garbage in Providence," by Dr. Charles V. Chapin, of Providence; "Refuse Disposal in Montreal," by Dr. E. Pelletier, of Montreal, and "Street Hygiene in Veracruz and the City of Mexico, by Dr. Manuel Iglesias, of Veracruz. (See pages 29-77.)

The Secretary presented the following resolutions in regard to the death of Major Walter Reed, U. S. A., which were offered by the Section on Bacteriology and Chemistry:

Dr. John W. Ross presented the following preambles and resolution, which were referred to the Executive Committee:

Whereas, It is well known that the previous excavations for an Isthmian canal have been attended with a large amount of illness and a very heavy mortality, due mainly to malaria and to yellow fever; and

WHEREAS, Recent discoveries as to the causes of these diseases have enabled medical officers to control their ravages to a marked degree; and

WHEREAS, There is every reason to believe that, if proper hygienic precautions be taken at the termini and at other stations along the route of the canal, the death rate of the past can be greatly decreased, or done away with, as has been evidenced by the success which has crowned medical efforts in Cuba, in abolishing yellow fever from that island; therefore, be it

Resolved, By the American Public Health Association, that most ample powers should be given to the medical officer in charge of the sanitation of the canal, and, that, in its judgment, the medical officer, or one of similar experience in hygiene, should be a member of the commission which the President of the United States is authorized to appoint to conduct the affairs of the proposed canal; it is, furthermore,

Resolved, That the Secretary of the American Public Health Association be instructed to forward this preamble and resolutions to the President of the United States. (Referred to the Executive Committee.)

DR. ARTHUR R. REYNOLDS, of Chicago.—Inasmuch as there are no more papers for this session, I move, Mr. President, we now adjourn until the afternoon session. Seconded.

DR. HEBER JONES, of Memphis, Tenn.—I would like to report, briefly, the results of our experience in Memphis in garbage disposal.

This is a question of grave importance. We have given a fair trial to the various methods of garbage disposal in Memphis. We have the Mississippi River in front of us, and for many years the offal from the city was hauled to the river and dumped therein. One might say that a river of that size would be an appropriate place into which to dump the garbage of a city, because, apparently, it would give very little offense when put into a stream as deep and as broad as the Mississippi is at Memphis. This, however, did not prove satisfactory. While the garbage was dumped from a dump-boat sixty feet long, and a large portion of the material sank to the bottom, other portions of the garbage floated off and were caught along the shore, where, being exposed to the rays of the sun, it became a source of offense and pollution to the air.

In 1898 we began to investigate cremation, with the result of our deciding to adopt a system of cremating everything, without any effort to save any of the offal of the city except ashes. The result of our trial has been this: The cost to the city of collections, hauling to the crematories, and the destruction of the garbage, amounts to about two-thirds of what it did when the garbage was hauled to the river and dumped. We have four crematories, so located that the longest haul in the city is about three-fourths of a mile haul. I am now referring to the thickly settled portion of the city. In the suburban districts, where the city is less thickly populated, the haul is somewhat longer, but the average haul is less than three-fourths of a mile because the four crematories are scattered over the city.

I was surprised at the detailed report made by Dr. Woodward for various cities as to the cost of collection and destruction of garbage by cremation. While I have no memorandum of the exact figures, I am perfectly familiar with the cost in Memphis. The cost of the collection of garbage is, at the outside, not over 75 cents per ton in winter, and in the summer season, when the garbage is heavy, the cost is 66 cents per ton. You can understand the reason of its being cheaper in summer, as the garbage is much heavier at that season, and of course may be collected more rapidly. This would give you an average cost for the year of not exceeding 70 cents per ton for collection.

Now as to the cost of incineration: I can show you beyond any possibility of a doubt, that we destroy the garbage in summer, when the crematories are run at their full capacity, for 15 cents per ton. In winter, when the crematories are not run at their full capacity, the cost does not exceed 20 cents per ton. We find it cheaper to run more crematories, even at half their capacity, than to haul the garbage long distances so as to run them at their full capacity. In summer, when our crematories are run at their full capacity, we destroy about 130 tons per day. The cost of labor is about the same in winter as in summer. The amount of garbage in winter is about one-half that of the summer season, hence the proportionate cost for incineration will be greater in winter than in summer. It is absolutely safe to say that the average cost of cremation of garbage in Memphis will not exceed 18 cents per ton the year round.

When I first put in crematories in the thickly settled portions of Memphis, a howl went up, and the people said they were going to destroy the value of the adjacent property. Of course, we were threatened with injunctions, but they did not come. During the first few months after the erection of the crematories, various complaints were made about them, but within the last four years not a solitary complaint has been heard. As to odors escaping from the stack, there is not one particle of trouble from this source. We have a combustion chamber at the rear of the furnace, in the bottom of the smokestack, which is intensely hot—being heated with coke—and this effectually destroys the gases. With reference to odors in the crematories, I can take any one of you into one, and, if you were blindfolded, you would never suspect you were there, the draught from the hole being so strong that the odor disappears immediately. It is sucked in, goes through the fiery furnace, goes up through the combustion chamber, and is destroyed.

I believe some gentleman stated that cremation had not been satisfactory anywhere. I want to say that there is not a particle of dissatisfaction expressed by anyone in our city, and you would be surprised at the small amount of coal it takes to run the crematories, constructed as they are. Of course, we have made many improvements on them since their original construction. We run the larger crematories—which have a capacity of from fifty to sixty tons per day—on a ton and a half of slack coal per day. If the garbage is wet this figure may be increased somewhat, but it will never exceed two tons. The crematories cool down between midnight and the time to start them in the morning, so it is necessary to use more fuel in the early hours, but after 10 or 11 o'clock in the day, little coal is necessary, and in the afternoon we use little or none.

The ashes are kept separate, and are used for filling unpaved streets. Such material is very useful, our city engineer claiming that it is worth about 20 cents per cubic yard. He estimates the value of the ashes we haul at over $4,000 a year, which is much more than it takes to run our crematories for the same period.

Unquestionably, gentlemen,—and I do not care what the size of the city is, whether it be New York, with its four million of people, or Memphis, with a little over one hundred thousand, the proper plan for garbage disposal is to destroy it by fire, and not try to utilize it for feeding swine, hauling it out and burying it, or making any attempt at reduction.

On motion, the Association then adjourned until 3 P. M.

FIRST DAY—AFTERNOON SESSION.

The Association reassembled at 3 P. M., and was called to order by the President.

In the absence of the Chairman of the Committee on Animal Diseases and Animal Food (Dr. D. E. Salmon, of Washington, D. C.), the report of this committee was read by Dr. M. P. Ravenel, of Philadelphia, Pa. (See page 78.)

Dr. Carlos John Finlay, of Havana, Cuba.—I move that a vote of thanks be extended to the Chairman of this committee, Dr. Salmon, for having conclusively demonstrated that bovine and human tuberculosis, from a sanitary point of view, are identical. Seconded by Dr. Montgomery, and carried.

Dr. E. A. de Schweinitz, of Washington, D. C.—Mr. President, and members of the Public Health Association: I should like to say a word or two in regard to one of the cases which Dr. Salmon has reported. It is the last case which he quotes from me, giving the result of intravenous inoculation of a culture obtained from the mesenteric glands of a child. At the time his paper was written, the results of the subcutaneous inoculation had not been obtained. They have been secured since, and we have succeeded in producing a generalized case of tuberculosis in a calf after the subcutaneous inoculation of five c. c of a suspension of a culture. The same quantity was used for intravenous inoculation, and the results were so positive that I have had a number of drawings made which show conclusively that it is possible for the tuberculosis germ obtained from a child to produce a generalized tuberculosis in cattle. Inoculations made intravenously produced the disease in seventeen days. Inoculation of the same quantity of material made subcutaneously produced in from twenty-eight to thirty days a greater amount of disease than that produced by a similar quantity of a bovine culture. I show you the two photographs. One shows the appearance of the lung of a calf that was inoculated subcutaneously with five c. c. of the culture of human origin.

In the same way, we have made experiments with other cultures. I do not know whether monkeys are our first cousins or not, but they come pretty near being so. We know that the blood of a rabbit injected with the blood of a monkey will give a precipitate with human blood, or the blood of a rabbit previously injected with human blood will give a precipitate with monkey blood or human blood, or vice versa, showing conclusively, it seems to me, that there is some clear relationship. You will see in these photographs the results obtained by inoculating monkeys with bovine cultures in one case, and with human cultures in the other case. This (referring to photograph) is the result from inoculation with the human culture, and the other from inoculation with the bovine culture. You will notice that the human culture has produced a much more generalized tuberculosis than the bovine culture.

Dr. M. P. Ravenel, of Philadelphia.—There is one point I would like to bring out more clearly, in which I think Dr. de Schweinitz agrees with me, namely, that the bacilli which we have found in the digestive tract of these children originated from a bovine source, so that we have direct proof that children are killed by tuberculous milk from cattle. It makes very little difference to us, as sanitarians, which view we take. My opinion is, based on the morphology and cultural characteristics, that these children were killed by the bovine tubercle bacilli.

Dr. Eduardo Licéaga, of Mexico, read a paper entitled "Sanitary Measures Proposed to the Mexican Railway Companies." (See page 101.)

DISCUSSION.

Dr. G. P. Conn, of Concord, N. H.—Mr. President: The Association is under obligation to Dr. Licéaga for his valuable paper describing what is being done in the Republic of Mexico. They are progressing in that Republic, and I believe that in the United States there is a change of opinion, and there is progress being made by the railroad authorities all over the country.

The student, in sanitation, when considering questions of railway sanitation, confronts two very important ideas—the ideal and the practical. The ideal in the sense of sanitation is the same as it is in our household effects—perfect cleanliness. It is almost impossible to carry that into effect. A railway car may start in as perfect condition as possible. It is filled with passengers; it encounters different degrees of temperature; the perspiration, and the respiratory efforts of the passengers are such that scarcely an hour goes by when the car becomes like an overcrowded room, devoid of true sanitation. We have to accept what is given us. Long rides are taken, and very little is done in the way of ventilation or cleansing cars. The practical part of it confronts the railway management, and only a few years ago they took but little notice of construction or anything else except what they termed for the comfort and pleasure of those who ride. Luxurious cars were provided, but little was done in the way of making them sanitary. A great deal was done to make the car look well, to beautify it with fancy paneling and bead-work, which, as every good housewife knows, is one of the most fatal things to ideal cleanliness in a house. Good plain walls, plain ceilings, plain floors, corners that are rounded and plain, constitute the easiest surfaces which can be made clean. I am informed that the Pullman Company are now constructing cars upon that principle, leaving out all of the fancy carving which collects dirt and renders the cars almost impossible of cleanliness.

I want to thank Dr. Licéaga again, for I believe his paper is the only one which is contemplated from the Committee on Car Sanitation. It is a subject which has been brought up many a time, and I believe that the education, which has come from the work of this Association, has had its effect upon the railway managers of the country. Less than ten years ago a railroad in New England, having between three thousand and four thousand miles of railroad, had less than four lines in their instructions to employes as to what to do of a hygienic character. Today their instructions to employes cover over two pages of their rules and regulations, and they insist upon these instructions being carried out as well as they know how. Some of the railroads are contemplating the establishment of schools that the employes may learn how to do this work. It is difficult to do this, for the reason that with the pay and the danger to life, the better classes of young men hardly care to enter the employment as trainmen. There are a great many of them who can read and write, which is considered a qualification to go into the railroad as trainmen. Such men must be instructed. Educational influence must be brought to bear, else they will not prove to be good sanitarians.

Dr. Licéaga spoke in regard to the bedding and other things, and queried whether they were disinfected before they were sent to a laundry. The Pennsylvania Railroad, I am informed, disinfects all of the linen of Pullman cars and all of the mattresses at the end of every trip. They have a disinfecting plant, whereby mattresses can be placed in an oven and subjected to sufficient heat for thorough disinfection. I am glad to know that so much is being done in our sister Republic (Mexico), because some of these days we shall want to go down there again, and then we will see the improvement which has been made during the past ten years. I thank Dr. Licéaga for his paper.

Dr. J. N. Hurty, of Indianapolis, Ind.—Too much emphasis cannot be placed on the care of sputum, but we must remember the soil must be prepared before the infection of consumption can take hold. It is the same with pneumonia. We cannot hope to cope with pneumonia by destroying the specific infection of that disease. We are told by bacteriologists that the infection is found in the oral and nasal cavities very frequently in seemingly perfectly healthy persons, and it is quite common to have it stated by those who are afflicted with pneumonia, that "I was on the cars very recently; I took cold, and it developed into pneumonia." I presume it is certainly true that any amount of care of the sputum would not have prevented the disease, and I think it is reasonable to suppose that the reason people have pneumonia is because their powers of resistance or life processes are lowered to an unusual degree by lack of fresh air. If that be true, it follows it is ventilation we want in railroad cars. We need that first, probably to a greater degree than cleanliness, although this does not disparage cleanliness in any degree. We must have them both; but it seems probable that if cars were ventilated properly, or if it were possible to get the full amount of fresh air, we would not hear this complaint so often by those who have colds, with pneumonia developing later.

When I was traveling on a Pullman car recently, a railroad man said to me that the lack of room makes it impossible for them to devise methods of ventilation. He said that room is at a premium, which is undoubtedly true. But, as one of the traveling public, I would prefer to pay an advanced price over what we pay now, large as it is, if we could have better air. I entered the car at Louisville; it was crowded; every berth was taken; every ventilator was shut tightly, and the air was dull, heavy and sodden. There was an excellent opportunity for one to take cold, and I have no doubt that some did catch cold. Fortunately, I did not. For my part, I cannot sleep in one of these little berths without raising the window a little, although I get a good deal of dust, smoke, and soot. It is not so injurious as foul air. So the crying need, after all, is for better ventilation.

Once before I spoke to this Association in regard to the bedding on Pullman cars. I think the linen is faultless. We know that to be so. I am very glad to learn from Dr. Conn today that the disinfection of mattresses is practiced by the Pennsylvania Railroad. I have been unable to discover it in our city at any of the terminals. We have a sleeping-car terminal at Indianapolis, and I doubt whether there is any disinfection of mattresses there. But I started to say, that I have referred to one thing before in this Association, and that is the blankets. The blankets are of the finest quality, very heavy, and of dark color. If we could demand of the sleeping-car companies white blankets, they would tell their own story of cleanliness just as the linen sheets do. Personally, I do not like the blankets that are in use at present; I cannot inspect them very well; I do not know what is on them; I do not know how filthy they are, or how clean they are; yet if we had white blankets, there could be a personal inspection.

DR. T. A. WILLIAMS, of Houston, Tex.—This question of car sanitation is one of great practical importance. I am an Englishman, and have only been in this country three months. During that time I have spent a great part of my time on your trains, and the conclusions I have reached I have communicated to medical men, newspaper men, and all kinds of people all over the country, with a view to agitating public sentiment to remedy the bad sanitary conditions of these cars. I was very much gratified in listening to the remarks of the last speakers, and those are my ideas *in toto.*

I will give you the contrast of two experiences. About six weeks ago I traveled from San Antonio to Austin, Tex., in a sleeping-car. My experience was the same as that of the last speaker, in that every ventilator was closed; the train was packed, and there were no vacant berths. The air was intensely oppressive and stuffy. It was in October, and we had a warm spell. I branched off to go from San Antonio to Houston, and when I entered the sleeping-car every ventilator was open; the purity of the air in the car was maintained during the whole night. There was a vast difference between it and the other car. The difference was that one car had an intelligent conductor, and the other car had a stupid, inefficient conductor. I believe the remedy lies with the public in insisting upon the companies establishing a training-school for all train conductors. I do not confine my remarks to Pullman cars. It is even more important that the matter should be attended to in the ordinary cars in which people ride who are not so competent to look after their own sanitation as the people who live and travel in Pullman cars. We see many men who are in the habit of spitting on the floor. There should be some regulation enforced by which a man is not allowed to spit in the cars except in a cuspidor, or the structure of the car should be modified, so that a man can spit out of the window by lowering the window sash, instead of having to raise it with difficulty, and very often it cannot be raised at all. I maintain, that in spite of the small cubic space which we have to put up with when the car is crowded, the point of toleration can be established in the atmosphere of the car, provided the conductor looks after the matter intelligently; and the important thing is the education of Pullman and other conductors, and I am glad to hear there are at least a few schools for such instruction.

Another point is the abominable and filthy habit of brushing one's clothes in the car by the porter. One's clothes are invariably dusty, and this dust may be septic, and still it is a frequent thing for the colored porter to brush the clothes of another man right near other passengers. This, to my mind, is a disgrace and should not be tolerated for a moment. There should be a special compartment in which the brushing of clothes of pasesngers should take place, or it should be done at the rear of the car. In order to do so, the public must be educated by an association such as this, and its absolute importance pointed out.

Dr. M. P. Ravenel, of Philadelphia.—There is one point which I have not heard mentioned either in the paper or in the discussion, that seems of importance. There is no provision in the toilet rooms of Pullman cars for cleaning the teeth. It is apparently the custom for men to clean their teeth and spit in the basins, which is disgusting from a purely esthetic standpoint, besides being dangerous to the health of others who come after. As Dr. Hurty has said, the pneumococcus and other disease-producing organisms are often found in the oral and nasal cavities. The basins are of bad construction, being made of metal and having an overhanging edge under which it is impossible to cleanse. The valves afford another receptacle for dirt, occupying a recess into which we cannot reach. The Pullman Company would do well to return to the old-fashioned rubber stopper in the center of the basin, as it does not furnish a recess for the accumulation of filth. There should be also a sink for cleaning the teeth provided.

Dr. Carlos J. Finlay, of Havana, Cuba.—I would like to say, that my recent experience with Pullman cars suggested to me the idea that it would be a great comfort to the passengers and an improvement from a sanitary point of view if, in addition to having eating-cars added to the sleeping-cars, there was a dressing-car. A better sanitary condition of the cars would be maintained; it would avoid brushing of clothes in the sleeping compartments, and would give more space for cleaning and washing of passengers.

Dr. John S. Fulton, of Baltimore, Md., read a paper entitled "Vital Statistics: A Plea for Actuarial Administration and Control of the Great Resources of Preventive Medicine." (See page 172.)

Dr. Cressy L. Wilbur, of Lansing, Mich., followed with a paper on "Coöperation Essential to Progress in Vital Statistics." (See page 198.)

Dr. Wilbur also presented the report of the Committee on Demography and Statistics in Their Sanitary Relation. (See report.)

These three papers were discussed jointly.

Dr. Liston H. Montgomery, of Chicago.—Mr. President: I was waiting to hear some other gentlemen speak on this subject and it is with some diffidence that I rise to discuss these papers.

Recently I have had an experience regarding classification and causes of death, which I would like to mention. Being the chief medical representative of a large accident insurance company in Chicago, a report was sent to the office of the company that the husband of a woman died, and the necessary blanks to fill out to prove the death loss were requested. We naturally supposed the cause of death was due to an accident. I immediately wrote to the registrar of the health department of the city where the death occurred, and requested a certified copy of the death certificate and the cause of death came back to me that the person in question died of "general debility." I made another effort to ascertain how long the deceased had been ill, etc., as the length of time was not stated. The reply stated "unknown," consequently that eliminated, as I judged, the cause of death as being due to an accident, which upon subsequent investigation proved that the man died from natural causes.

Another case I might cite, where death was reported as being due to septicemia—simply that, and nothing else, no cause having been assigned as to what produced it. Investigation proved that the patient, a woman, had died of this malady after three days' illness.

It seems to me, those two instances and many others that might be mentioned by other speakers, are enough to show that such causes of death should *not* be accepted by registrars and vital statisticians.

DR. WALTER D. GREENE, of Buffalo, N. Y.—Dr. Fulton, in his paper, made the statement that in any city that had a death-rate lower than seventeen per thousand there must be something wrong with the figures. I feel that I ought to take issue with that statement. The city in which I live, Buffalo, N. Y., according to the government census in 1900, had a population of 354,000. One year later we placed our estimate at 375,000, thinking in a city of that size we had grown about twenty thousand. We take into consideration every death that occurs in the city. Now, in a city of that size, with its geographical position, with people entering the hospitals from the West of New York, Eastern Ohio, Northern Pennsylvania, etc., we have a great many deaths in our hospitals of persons who do not belong to us; but they are counted in our death-rate, and by careful figuring, trying to be accurate, we do not make our death-rate above fourteen per thousand, and for that reason I feel that Dr. Fulton has made a mistake in reference to Buffalo.

DR. MARCUS HAASE, of Memphis, Tenn.—We have to take a little of the medicine that has been offered by Dr. Fulton, but not so much, in regard to the reporting of deaths that occur within the city of

Memphis. This we positively do, irrespective of the value or worth that may be attached to such statistics. While Dr. Fulton did not mention Memphis or any municipalities in particular, I desire to say that all the deaths that occur in the city of Memphis are reported, and nothing is excluded, not even premature births. That is also true of the city of New Orleans. No death occurs in the city of New Orleans which is excluded from their monthly and annual reports.

As regards the population, I think all cities would like to have some plan by which they can estimate the population after the census is given to us each ten years. We estimated our population for the year 1901 at 107,500; while the census of 1900 gave us 102,320. Our death-rate for the year 1900, based on the population given us by the United States census, was 17.15 per thousand. We did not go below Dr. Fulton's seventeen.

In regard to Bulletin 71, I consider it one of the most valuable ever issued by any of the United States departments for the benefit of health departments, or for municipal and state registration offices.

In regard to the paper presented by Dr. Wilbur, I desire to thank him very much for it, especially for that part of it in which he speaks of the relative value of two given causes of death. Let us take, for instance, cirrhosis of the liver and malarial fever. Malarial fever in the city of Memphis would be of greater importance than cirrhosis of the liver. Malarial fever would receive a more prominent place in the classification than cirrhosis of the liver. Malarial fever would doubtless be placed as ten in the classification, while cirrhosis of the liver would drop down. I believe this classification, if adopted by all municipal and state registration offices, will be of invaluable service.

Dr. Carlos J. Finlay, of Havana, Cuba.—I would like to call attention to some observations I have made in connection with the reports of statistics of mortality in the city of Havana. It is important that there should be uniformity in the mode of computing the annual mortality per thousand, and that the mode of computation should be accurate.

When I took charge of the Sanitary Department of Havana, I found that the methods which were in use were too empirical. I therefore substituted a formula which is mathematically correct. It consists in taking as a basis the average daily mortality of the month, and multiplying that by a coefficient which is obtained by dividing 365,000 by the figure expressing the population. This formula gives the exact death-rate per thousand inhabitants in the year of 365 days. At the same time, it is important that a uniform formula should be adopted by all, so as to avoid confusion in comparing mortality reports from different sources.

Another innovation which I introduced was to compare the number of deaths from each disease, in a given period of time, with those in the preceding equal number of days. If we take the period of eight days adopted by the Marine Hospital Service, that period, for practical purposes, is found to be too short. I was anxious therefore to obtain a method by which the deaths in a given month could be accurately compared with those in the same number of days in the preceding month. This is easily obtained by comparing, for instance, the deaths in a month of thirty days with those corresponding to the last thirty days of a preceding month of thirty-one. By that means we can get a pretty accurate idea of the increase or decrease in the mortality for a given disease.

I would suggest, particularly, that boards of health come to some agreement as to the formula by which the death-rate per thousand shall be computed.

Dr. Fulton (closing the discussion).—If I were health officer of a city of 100,000 inhabitants or over, and forty years old or older, and my mortality statistics should show a death-rate of less than seventeen per thousand, I should feel myself between the devil and the deep sea. Whether the figures were right or wrong the circumstance would be disquieting; if the figures are right, too bad for the city, if the figures are wrong, too bad for the mortality registration. On the whole one would prefer to discredit the figures, though one should never enter into any statistical study with a prejudice or preconception.

At any rate I should examine first the figures, and I might find that out-of-town deaths of citizens had been lost to the mortality account, or that deaths in town had escaped either the count or the record, or that the population had been overestimated, or that some other error had occurred in the registration of mortality. But if I were convinced that the figures were correct, I should say "Too bad. This town is yet an infant city." Such a city cannot have a normal population as to age or sex. The population does not recruit itself in a normal way by natural growth. There is a disproportionate influx of healthy adults without families. People are leaving the town with the rewards of their work, to die elsewhere. There are few old people. There are few babies.

With reference to the proclaimed death-rate of fourteen per thousand in Buffalo, I should say that Buffalo lays claim to an especially light infant mortality. The production of infants in Buffalo may be relatively light. This is worth inquiry. The sources of Buffalo's growth are probably not in the birth-rate. The gentleman from Buffalo has told us that he divides his mortality by the estimated population at the end of the year. This gives him the advantage of the growth of the town in the last six months of the year. He should take the mean population for the year as his divisor. But the point is that death-rates are of little value in a population whose age distribution is unknown, and that the just credit of sanitation is generally to be found upon the fraction side of the death-rate.

The report of the Committee on "Cause, Prevention, Period of Incubation and Duration of Infectious Diseases" was read by title, in the absence of the Chairman, Dr. A. Walter Suiter, of Herkimer, N. Y. (See page 103.)

Dr. Luis E. Ruiz, of Mexico, read a paper entitled "Period in which Every Contagious Malady can be Transmitted, and Period in which Every Sick Person is Dangerous to Healthy Persons Near Him." (See page 114.)

Dr. José P. Gayon, of Mexico, followed with a paper entitled "A Note Concerning the Transmission of Pathogenic Fungi by Flies and Mosquitoes." (See page 116.)

On motion, the Association then adjourned until 8 P. M.

FIRST DAY—EVENING SESSION.

The Association reassembled at 8 P. M., and was called to order by the President.

Prayer was offered by Rev. Father Laval, of the St. Louis Cathedral.

The President then introduced his honor, Mayor Paul Capdevielle, who delivered an address of welcome on behalf of the city.

An address of welcome on behalf of the state was delivered by Hon. J. Y. Sanders, Speaker of the House of Representatives of the State of Louisiana, in the absence of his excellency, Lieutenant-Governor Albert Estopinal. (See page 16.)

At this juncture, Dr. Henry P. Walcott, of Cambridge, Mass., took the chair, and introduced the President, Dr. Henry D. Holton, who then delivered his annual address, after which the Association, on motion, adjourned until Wednesday, 10 A. M. (For annual address of President, see page 1.)

DECEMBER 10—SECOND DAY—MORNING SESSION.

The Association met at 10 A. M., and was called to order by the President.

After some announcements by the Chairman of the Local Committee of Arrangements, the Secretary presented the following resolution, which was adopted by the Executive Committee and recommended to the Association for similar action:

Resolved, That the Republic of Cuba be included among the nationalities entitled to representation in this Association.

Resolved, That the Secretary be instructed to send greeting to the President and health authorities of Cuba on the part of this Association, and to request that delegates be accredited the Association from that country.

THE PRESIDENT.—What disposition will you make of these resolutions?

DR. LISTON H. MONTGOMERY.—I move that they be adopted. Seconded by Dr. Wilkinson, and carried.

THE SECRETARY.—The following preamble and resolutions, offered by the Committee on Demography and Statistics in Their Sanitary Relations, have been considered by the Executive Committee, and are reported back to the Association with the recommendation that they be adopted:

WHEREAS, The American Public Health Association, at its meeting at Ottawa, September 27-30, 1898, unanimously passed the following resolutions:

Resolved, That the American Public Health Association recommends that the Bertillon classification of causes of death be adopted by all the registrars of vital statistics in the United States, Canada and Mexico, as soon as the change from the systems now in use can be conveniently made;

Resolved, That a proposal be made for an international alliance between the registrars of the three countries included in this Association, and the registrars of France and other countries now using, or which shall hereafter adopt, the Bertillon system, and that definite plans for such an alliance shall be submitted for action to the next annual meeting of this Association;

Resolved, That the governments of the United States, Canada and Mexico be likewise requested to make this classification the basis of the mortality statistics of the censuses of 1900 and 1901; and

WHEREAS, The action recommended by this Association in the above resolutions has been generally accepted and, as far as possible, thoroughly carried out; therefore, be it

Resolved, That the American Public Health Association heartily congratulates the registration offices of Maine, New Hampshire, Vermont, Massachusetts, Rhode Island, Connecticut, New York, New Jersey, Maryland, Pennsylvania, District of Columbia, Michigan, Indiana, Illinois, Wisconsin, Minnesota, and other states, including all of the registration states in the Union, the Provinces of Ontario and Quebec, and the Superior Board of Health and States of Mexico, and also the municipal registration offices, upon their adoption of the uniform classification thus recommended, which, since its revision at Paris in 1900, is known as the international classification;

Resolved, That cordial acknowledgment be extended to the International Statistical Institute for its action at Christiana in 1899 in support of the plan of revision proposed by this Association; to the French government for its services in convoking the International Commission of Revision at Paris in 1900; and that the earnest coöperation of the American Public Health Association be pledged to the representatives of the twenty-six countries there assembled in securing adherence to the principles of a uniform and comparable international classification of causes of death;

Resolved, That the thanks of the American Public Health Association are due to the United States Public Health and Marine Hospital Service for promptly publishing and distributing among American registrars the first translation into English of the international classification of 1900;

Resolved, That the thanks of the American Public Health Association be especially extended to the census offices of Canada, Mexico and the United States for their prompt adoption of the international classification, and to the United States Census Office in particular for preparing and furnishing to the registrars of that country its recently published "Manual of International Classification of Causes of Death," whose general use, as a most convenient and accessible practical basis of uniformity, is recommended to all English-speaking registration offices embraced in this Association;

Resolved, That the efforts of the Committee on Demography and Statistics of this Association in conjunction with the United States Census Office to secure the extension of the registration area by the enactment of suitable laws and the use of a standard form of certificate of death, as shown in Census Circular No. 71, be heartily commended, and that the committee be further authorized and directed to coöperate with the Census Office, the United States Public Health and Marine Hospital Service and other departments of the federal government interested in vital statistics, and with similar committees from other associations in the work of promoting the adoption of suitable registration laws and the extension of the registration area, the proper compilation and presentation of vital statistics by states and cities in weekly or monthly bulletins and annual reports, and also in further work relating to the extension and practical use of the international classification of causes of death, the disposition of jointly returned causes and all preliminary work relating to the next decennial revision;

Resolved, That all registration offices in the countries included in this Association that have not already adopted the international classification be urged to do so, on the ground that the advantages of general uniformity outweigh any local preference.

The President.—You have heard the report of the Executive Committee, as presented by the Secretary, recommending the adoption of these resolutions that have been read. What is your pleasure?

Dr. J. N. McCormack.—I move that the resolutions be adopted. Seconded and carried.

The Secretary presented a communication from the New Haven County Public Health Association, sending greetings to the American Public Health Association, signed by Professor Brewer and others.

Mr. Henry A. Haigh presented the following resolutions:

As a representative in this body from the State of Michigan, I feel it my duty to the memory of a distinguished sanitarian late of that state to call attention to the announcement so feelingly and appropriately made in the President's masterly annual address, that on the seventh day of November last, there passed from a sphere of intense and lifelong usefulness, one of the early Presidents of this Association, and one of the pioneer apostles of preventive medicine in this country—Robert Clark Kedzie, M. D., LL.D.

It seems fitting that this Association should take some formal notice of the passing away of this devoted disciple of the public health, and I have therefore prepared and beg to submit the following resolutions:

Resolved, That the American Public Health Association has received with deep sorrow the intelligence of the death of Dr. Robert Clark Kedzie, one of its early Presidents, and a lifelong, practical and indefatigable worker in the field of public health;

Resolved, That this Association places a very high value upon the unselfish and untiring labors of Dr. Kedzie in behalf of sanitation and the public welfare, and recognizes in him a superb type of the hardy hopeful pioneer in preventive medicine, the reward of whose devotion is being and will be reaped in better health and happiness by this generation and by generations yet to come.

Resolved, That these resolutions be spread upon the records of this Association as an expression of our esteem for Dr. Kedzie as a man and sanitarian, and of our belief in the high value of his life to the science of sanitation and to the human race.

(Referred to the Executive Committee.)

Dr. Heber Jones.—I understand that quite a number of the members are going to stop over in Memphis on Sunday next. If so, I would like to extend a cordial invitation to them to inspect our crematories when in operation. Ordinarily, we do not run them on Sunday, but if you will let me know about what time you will be there, I assure you the crematories will be in operation.

The President.—The next thing in order is the report of the Chairman of the Committee on Public Health Legislation, by Dr. U. O. B. Wingate, of Milwaukee, Wis. (See report, page 118.)

At the conclusion of his report, Dr. Wingate said: The Committee on Legislation should be continued, for new legislation is coming up and many bills are being introduced in Congress which need watching, especially such bills as that of the anti-vivisection bill, which is liable to come up again. There is much need of a legislative committee to prevent, if possible, bad legislation, and to favor any legislation that may be of interest and advantage to the progress of sanitation.

Dr. J. N. McCormack.—I move the adoption of the report of the committee; that the committee be continued, and in connection with that motion, I desire to move, sir, if it be in order, that this Association pledges its support and coöperation to the Marine Hospital Service, in order that it may make every effort toward improving the public health service and conditions of this country. Seconded and carried.

Dr. Walter Wyman.—May I be accorded the privileges of the floor for a moment, Mr. President?

The President.—You may.

Dr. Wyman (continuing).—I shall speak but a moment or two. I feel that I should not let this occasion go by without expressing the gratification which this last resolution has given me. I have heard the report just read expressing such kind terms of good feeling towards this new service. I wish to express my appreciation also of the kind words uttered by the President last night in his annual address, extending his congratulations to this new service, and to myself at the head of it. I wish to assure this Association that the spirit in which this bill was framed was one entirely in accord with the spirit which prevails throughout this Association; that the time has come when I believe there will be no more quarreling in public health matters; that the work is to be one of coöperation, consideration for one another, and that is the sentiment which shall prevail in the administration, so far as I have anything to do with it, under this new service. (Applause.)

THE PRESIDENT.—The next subject for consideration is the "Cause and Prevention of Infant Mortality," by Dr. Francisco Sanchez Munoz, of Mexico. (See page 302.)

Dr. Ramon N. Prado, of Mexico, contributed a paper on "Wet Nurses: Their Hygienic Importance." (See page 307.)

DR. FELIX FORMENTO.—I move that the President's address be referred to a special committee. Seconded and carried.

The report of the Committee on Disinfectants and Disinfection was read by Dr. H. W. Hill, of Boston, in the absence of the Chairman of that committee. (See page 207.)

Dr. H. W. Hill, of Boston, read a paper entitled "Examination of Rooms Occupied by Diphtheria and Tuberculous Patients for the Presence of Bacillus Diphtheriæ and Bacillus Tuberculosis Respectively." (See page 209.)

Dr. M. P. Ravenel, of Philadelphia, followed with a paper entitled "Experiments in Disinfection with Formaldehyde Gas." (See page 221.)

These three papers were discussed jointly.

DISCUSSION.

DR. SMITH, of Porto Rico.—Mr. President: May I crave the indulgence of the Association for a few moments on a point which to us, in Porto Rico, is of great interest, and may prove the same to you? In the twelve months, ending last July, in Porto Rico we had thirteen deaths in human subjects from glanders. Glanders prevails enormously in Porto Rico, and urgent measures are necessary to stamp out this disease, and among the measures to prevent the spread of the disease, disinfection of one kind or another was found essential. Those of you, who are acquainted at all with the conditions in Porto Rico know that expensive apparatus, such as generators, etc., for purposes of disinfection, are utterly impracticable, on account of financial reasons. The buildings occupied as stables, and such like, are utterly incapable of being properly sealed for the use of such disinfection. I want to bring to your notice a simple means which we have found efficacious, and it may not be known to you. We used it extensively in the army service in Ponce and San Juan, at the time when the army horses and mules were infected. The walls and floors were brushed and flushed with kerosene oil, then igniting the kerosene, brushing and flushing again, and burning off the surface by repeating this process on the walls and floors. In this way we disinfected rooms without destroying the building. Of course, this flushing process required care, and larger places were gone over with other germicides. That plan has been found uniformly successful in the case of infected stable buildings, infected with glanders. We have used the method in many instances in houses in which there were tuberculous patients. We had five consecutive deaths in eleven months in one house infected with tuberculosis. We went through this process in that house, and for eighteen months there was not another case.

Dr. Charles S. Caverly, of Rutland, Vt.—In listening to the practical and interesting paper of Dr. Hill on tuberculosis and diphtheria, it occurred to me that the usefulness and value of his experiments would be increased if he had studied (and it is barely possible he did study) the exact conditions that had prevailed during the sickness of the diphtheria or tuberculous cases and the conditions of the room.

I wish to ask Dr. Hill whether these cases were taken care of in a modern way? Was the sputum carefully destroyed? Were the rooms prepared for the patients, particularly the diphtheria patients, before they were allowed to occupy them?

Dr. Adolph Gehrmann, of Chicago.—I wish to speak of two points in this discussion. First, in regard to the difference between the results in diphtheria and tuberculosis. I am quite convinced that under modern sanitary control a case of diphtheria could remain in a room without any particular danger, if cared for after our present modern ideas.

In regard to tuberculosis, such control is much more difficult, because, as a general thing, these patients depend more upon themselves and their own arrangements as to their care, and the distribution of tubercle bacilli is more apt to occur than the distribution of diphtheria bacilli from cases of that disease. It brings us to the one point that the patient himself is really the dangerous factor. In diphtheria physicians recognize this as shown in the necessity of covering the face in making all intubations, because particles may be thrown out or expelled by the patient, thus causing infection of the physician. Furthermore, in cases of tuberculosis, a glass plate has been held up before a tuberculous patient when coughing, showing particles of sputum on it containing tubercle bacilli, which could be stained. Klebs has made such a demonstration. The sputum receives more attention in diphtheria than in tuberculosis.

In regard to the second point, I desire to call attention to the fact that the Chicago Health Department has been using the method of spontaneous evolution of formaldehyde for about four years, and we are convinced that we are having success with that method.

As to the use of blankets, I do not believe a blanket is a good medium upon which to spray formaldehyde. Furthermore, I do not believe that method is as good a one where the formaldehyde is diluted as where it is concentrated. The idea is to use as small drops as possible, in order to allow spontaneous evolution to take place. A room of this size, for instance, could not be disinfected, unless the disinfectors wear masks, such as are used by firemen to prevent danger from smoke or fumes. Disinfection begins almost the moment the formaldehyde is sprinkled upon the sheets, and the disinfection continues practically as long as the room is closed. It is a continuous, rapid method of disinfection, and in the experiments we have made, and I have reported some on several occasions, I have been convinced we are getting generally as good a series of results as can be obtained under the same circumstances with other methods or devices.

DR. F. F. WESBROOK, of Minneapolis, Minn.—The experiments of Dr. Hill have been most interesting in their results, and he has been extremely conservative in his summary. He has advanced certain facts, and has shown their application to the manner of disinfection. Furthermore, the information was obtained through a particular channel, and, as he has indicated, those are not the only channels through which we may obtain information. This work of Dr. Hill would seem to show that in diphtheria, so far as his experiments can be taken to prove the point, the method of infection was not so much by means of clothes or beds, or articles of that kind, but that it must have been by nose or throat infection, which would seem to be borne out by other observations.

In Minnesota we had last year in a town of about one thousand inhabitants an epidemic of diphtheria during the winter, which did not disappear in the spring. It was present in the summer, so that the schools had to be closed before their regular time, and as diphtheria existed throughout the course of the summer, the authorities were very much exercised to know what to do relative to opening the schools in the fall, when widespread infection was expected. They therefore interested themselves in the matter, and provided a place where the clinical cases could be housed, and where they were kept until the danger of infection was gone, as shown by an examination of the secretions from their noses and throats. Although this is not always practicable, an opportunity was afforded to make an experiment, and so all the children of the schools who were shown to have nose or throat infection from the typical forms of bacilli, were excluded. I do not know whether it was the result of that, or what the reason was, but diphtheria died out at a time of the year when one would have expected to see it increase rather than diminish. This, it seems to me, bears somewhat upon the same subject, and indicates that the means of infection was by direct transmission more frequently than by any other medium in this one disease, but this should not lead us to be less careful in regard to disinfection.

I believe the disease is more frequently communicated by contact from person to person, than by means of clothing, furniture, or some other similar means.

Dr. H. M. Bracken, of Minneapolis, Minn.—The executive officers connected with this Association are under obligation to Dr. Hill for his excellent paper. We are faulty in matters pertaining to quarantine and disinfection, and as we find from practical demonstration that we can ease up in our quarantine and disinfection regulations, we should do so; at the same time, care should be taken to guard against the actual dangers. Dr. Wesbrook has illustrated very well some of these dangers. The dangers from diphtheria are emphasized by our experience since the introduction of antitoxin. For while this agent has reduced the mortality very materially it has also, to a certain extent, brought about carelessness in the methods of dealing with the disease, and I think we can say that in some districts, at least, there is more diphtheria at present than formerly because of this carelessness. It is fortunate indeed that the direct infection from diphtheria has been brought out so clearly and thoroughly by Dr. Hill. I am glad to learn that the danger from bedding and clothing connected with diphtheria patients is so slight. Direct transmission of infection is certainly the point we should keep in mind in dealing with this disease.

Dr. J. J. Kinyoun, Glenolden, Pa.—I desire to call attention to some unpublished observations which I have made on the subject which Dr. Hill has so clearly set forth. It was my good fortune to be able to make an extended series of observations on the bacteriological condition of the railway coach. These observations were made during 1897 and 1899, continuing over a period of two and one-half years. When I undertook this work I held a preconceived notion that the railway coach was a charnel house filled with tuberculosis, diphtheria and other bacteria injurious to man. Over three hundred separate examinations were made of the interiors of railway coaches—first these were to determine the number of bacteria suspended in the air of a given number of cubic feet, and then to determine the number of bacteria which could be removed from a given area of cushions, mattresses, bedding, etc. I collected the samples from the air by a special apparatus, and those collected from cushions, bedding, etc., were in the same manner as described by Dr. Hill, only that the swabs were larger and were impregnated with a slight amount of diluted glycerin. The samples thus collected were made in the caryards at Jersey City, in Philadelphia, in Washington and at other terminal points in this country. It was necessary to collect many of the samples enroute, particularly those for determining number of bacteria per cubic foot of air. In over three hundred examinations made I found the bacillus tuberculosis in three instances, diphtheria in five instances and the organism of pneumonia as many as twenty times at least. The tubercle bacillus was not found in the air, nor on the bedding. In one instance it was recovered from the carpet near where the cuspidors are placed, and in two others on the plush of the seat. The few times in which bacillus tuberculosis was found in cars occupied by tuberculosis patients, many of whom were expectorating large quantities of sputum rich in tubercle bacilli, was somewhat of a surprise to me, and I must admit somewhat of a disappointment in view of my preconceived notions of the dangers from this source. I would state, in this connection, that these observations were made with special reference to the danger of tuberculosis and confined to cars enroute to the several health resorts, and I attempted so far as possible to keep notes on the general condition of the persons traveling therein. Finally I would say, I find that the drinking cups in cars are a great source of danger, particularly with regard to diphtheria, as nearly all the diphtheria bacilli isolated came from this source. The observations which I made are presented for just what they are worth. I do not think the subject of this investigation is by any means complete. I, myself, am not satisfied with the results, and wish to continue them somewhat further before giving a final opinion on them.

Dr. Hill (closing the discussion on his part.)—In reply to the first question as to the conditions of the rooms, they had not been particularly prepared in any way. In the private tuberculous rooms examined, we were unable to discover any spittoons in any one of the houses, but in the hospital the greatest care was taken to use spittoons, and particular attention was given in general to the hygienic care of the surroundings. A friend of mine, Dr. J. H. Elliott, who did some similar work in Liverpool, told me that he found a certain relation between the abundance of tubercle bacilli scattered about the room and the use of spittoons. We, however, have not found such relation. I do not think too much stress should be laid on the sputum.

The remarks of Dr. Wesbrook were admirable. Our results confirm what he has said regarding the infectiveness of diphtheria, namely, that the infection is from patient to patient, by direct contact, and not from clothes, bedding, etc. The infection is conveyed by close contact, such as children tumbling over each other on the floor.

I believe Dr. Kinyoun stated that in his three hundred examinations, he found diphtheria bacilli in only five instances. I would like to ask him whether those bacilli were virulent; whether they were truly diphtheria bacilli, and whether, if they were, he could account for the presence of the bacilli by any history of diphtheria patients in that car, and whether he has any means of knowing whether these germs were deposited by sick or by well persons.

Dr. Kinyoun.—I could not tell. Four of the cases had diphtheria bacilli from drinking cups, and the other from a drawing room. I believe they were virulent, and were tested with antitoxin.

Dr. Ravenel (closing the discussion).—I have only a word or two to say in regard to the remarks of Dr. Gehrmann, concerning the use of blankets. We have not considered them as the best material on which to spray formaldehyde. As we had to disinfect an enormous brewery stable in which there were many blankets and no sheets, we used horse blankets with good results, but the sheet is unquestionably the better method.

Dr. H. M. Bracken, of Minneapolis, Minn., Chairman, presented the report of the Committee on National Leper Homes. (See page 123.)

In connection with the report, Dr. Bracken said: I would say incidentally that Minnesota felt comparatively safe as regards leprosy up to the last year or two. We felt that there were no American-born lepers in that State. However, on investigation, we have found, during the last eighteen months, three American-born lepers, and that makes leprosy still more interesting for our State.

On motion, the Association then adjourned until 8 p. m.

Note. In the afternoon the Association and its guests availed themselves of an excursion given on the steamboat Lotus Sims to Kenilworth plantation.

SECOND DAY—EVENING SESSION.

The Association reassembled at 8 P. M., and was called to order by Henry P. Walcott, of Cambridge, Mass.

Dr. Eduardo Licéaga, of Mexico, presented his "Annual Report on Yellow Fever in Mexico." (See page 258.)

Dr. Narciso del Rio, of Mexico, read a paper entitled "Information on the Orizaba (Mexico) Yellow Fever Epidemic." (See page 262.)

Dr. John W. Ross, United States Navy, read a paper entitled "Reasons for Believing that the Only Way in Nature for Yellow Fever to be Contracted by Man is From the Mosquito." (See page 247.)

Dr. William C. Gorgas, United States Army, contributed a paper entitled "Concerning the Method of Transmission of Yellow Fever from Man to Man," which was read by Dr. Stark, in the absence of the author. (See page 238.)

Dr. Manuel S. Iglesias, of Mexico, followed with a paper entitled "The Disinfection of Railroad Cars as a Precautionary Measure Against the Propagation of Yellow Fever by Mosquitoes." (See page 267.)

The President invited Drs. Juan Guiteras and Carlos J. Finlay to take seats on the platform.

The President announced the symposium on yellow fever as being open for discussion, but as the hour was somewhat late, Dr. Edmond Souchon moved that the Association adjourn, and that the discussion of the papers be made the first order at 10 o'clock sharp, Thursday. Seconded.

The President, after putting the motion to adjourn, declared it voted down, after which the discussion was proceeded with.

DISCUSSION.

DR. CARLOS J. FINLAY, of Havana, Cuba.—Mr. President: I wish to say a few words in regard to those papers. In the first place, I wish to thank Major Gorgas and Dr. Ross for their flattering phrases.

One of the first things that led me to a successful investigation of the etiology of yellow fever was the admirable work done by the Yellow Fever Commission, presided over by Professor Stanford Chaillé, which was sent to Havana in 1879, and in which Dr. Sternberg, Dr.

Guiteras and others took an active part. I believe that the work of this commission constituted the first step in the right direction towards a clear understanding of the etiology of yellow fever. It was the report that was sent to me in 1880 which made the scales fall from my eyes and set me on the right track. For twenty years I had been investigating the air of Havana for the cause of yellow fever, and I had felt persuaded that I was on the right track. I had discovered a remarkable alkalinity in the air of Havana, of a peculiar kind, and many experts in different parts of the world considered my findings of some importance. But, I must say, as soon as I read the report given by that commission I became convinced that I was wrong; that the cause of yellow fever could not be in the atmosphere; that the disease was transmitted from one patient to the next. The findings of the Yellow Fever Commission of 1879 convinced me that a specific germ which could only be communicated by inoculation was the cause of the disease, and that the inoculation had to be performed by a special insect, which I soon afterwards identified as the one now called *Stegomyia fasciata.* I believe therefore that the report of the Yellow Fever Commission of 1879, communicating the observations of this commission, was the first to throw a full light upon the question of the etiology of yellow fever.

Dr. Edmond Souchon, of New Orleans, La.—I do not think anybody, who has given the least attention to the study of the transmission of yellow fever, doubts for a moment that the mosquito is the transmitter of the disease. The labors of the now immortal Reed have proven that beyond a doubt. It is not a mosquito theory; it is a mosquito demonstration. We all admit that as a fact. There is no going beyond it. It is settled. The energetic work of Gorgas in Havana has also settled the point that it is the first duty of a health officer in any community, where there is any possibility of any case being one of yellow fever, to destroy all the mosquitoes that are in sight. These two points are admitted by everybody. We always accept facts that are demonstrated, as treasures, and while we admit that the mosquito will convey yellow fever, we are not as yet prepared to say that it is the only means of transmission of this disease. We do not say it is fomites. We simply, modestly, honestly say that we do not know exactly what it is. But from the facts that are before us, there must be some other means of transmitting the disease. The labors and researches of these remarkable men made a deep impression upon me, as upon many others. But I have been through many epidemics of yellow fever, dating back to my introduction into the medical profession in New Orleans in 1867. I remember very well cases that impressed me at the time, and which again reimpressed me. I went over the literature; I went over the records of the board of health of the quarantine stations; I went over those archives of the great Mexican Gulf, where all these experiments by nature are all the time going on, and I gathered such a number of cases that I published them in an article in the Medical Record of New York. They led me to think there must be some other cause of transmission of the disease, fomites or something else, I did not know positively. Gentlemen, those cases, to my mind, and to the minds of many others, are so important that they cannot be overlooked; they are based upon facts admitted by Reed himself, confirmed by the experiments of others. Mosquitoes will not live more than five days without water, and all these cases I cannot explain under such conditions. If time permitted, I should take great pleasure in reading these cases, but I will skip them and call your attention to some other facts in connection with these experiments, experiments which have been recorded in regard to fomites and the mosquito in Havana by the American Yellow Fever Commission, later on by Dr. Gorgas, and others, and subsequenttly confirmed. The experiments were conducted under certain conditions, but there may have been some things that they left out. One of the first things that they have left out is the time of the year when they made the experiments. All the cases I report are instances I consider to be experiments made by nature herself, and she has left nothing out. We may depend upon that. As for the experiments made by my friend and former pupil, Dr. Ross, in September, that was the proper time, it is true; but, gentlemen, Reed himself wrote to me at the time that 33 per cent. of the supposedly non-immune patients, bitten by supposedly infected mosquitoes, did not develop yellow fever, and how can we expect all experiments with fomites to develop the disease. Having failed with the mosquito, it is only natural to suppose that we should fail sometimes with fomites also.

Now, we are told that the mosquito (stegomyia fasciata) is the only transmitter of yellow fever; that all our efforts should be concentrated on or against the mosquito. We believe, of course, in killing the mosquito, but we believe in doing more than that. In the South we have suffered a great deal from quarantine methods. We do not want to go through another epidemic of yellow fever, with all its horrors; but we want to demonstrate, if possible, that there is another cause of transmission of the disease. We want to be slow in attempting or enforcing radical measures. We want to be sure what we are doing. It will be time enough to change our methods later on. It is a very small expense and trouble to detain a vessel long enough to disinfect the baggage and clothing. But what we want is additional proof regarding some other mode of transmission of the disease than the mosquito. We are all open to conviction. We are all scientific men. We all desire to advance, but we want to advance slowly, and take no risk that may precipitate upon our people calamities such as they have gone through three or four years ago. We are not set in our ideas, by any means, but all we want is more light; all we want is demonstration, and the moment we have it, we will be the first to accept it, but it must be clear, positive, and safe.

Dr. A. N. Stark, U. S. Army, West Point, N. Y.:

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Like Dr. Souchen, I fully appreciate that nature leaves nothing out: in her wise conservatism she does not seek two roads to attain the same goal.

Those who believe that yellow fever can be communicated through more than one channel must be consistent, and believe also in the "water borne" theory of malaria. The two diseases—yellow fever and malaria—are analogous. I was a doubting Thomas myself until the conviction was forced upon me that the mosquito, and that insect alone, was capable of conveying from man to man the disease from which the southern people of the United States have so dreadfully suffered.

When the experiments upon human beings were made possible for the last time in the city of Havana, they were open to the world. They were for the benefit of the states bordering upon the Gulf of Mexico. To my certain knowledge, the only southern health officer who availed himself of the opportunity to study these experiments was Dr. Porter, of Florida.

The scientific world, in forming its judgment and in promulgating its conclusions, demands facts and not words, and never will be influenced by monographs, though they make a pile as high as the Tower of Babel, unless they are supported by direct experiments.

In his article, published in the Medical Record of October 25, Dr. Souchon deals with the treatment of vessels from yellow fever ports. A "long trip" sailing vessel from Rio Janeiro, having arrived at quarantine, yellow fever appeared among the crew a few days after. The solution of this is simple. I think that all of you gentlemen who are interested in the quarantine question will remember that the holds of wooden vessels contain large quantities of bilge water, and in my native town I have seen large numbers of mosquitoes circulating in the holds of these vessels and myriads of larvæ in their bilges. As the vessels approach the quarantine station the hatches are thrown open in preparation for inspection by quarantine and custom house officials.

The hatches open, the insects, maddened by their confinement and hunger, rush forth and feed upon the unsuspecting crew, and shortly after their arrival at quarantine yellow fever appears.

The fact that the mosquito hibernates can be well proven in the northern parts of the United States. For instance, it is well known that mosquitoes hibernate on the plains of Montana and Dakota where it is very cold in winter, and they are abundant in these places in the summer.

How the mosquito hibernates under such conditions, experimentation has failed to show.

Today I wrestled in prayer with my friend, Dr. Porter, of Florida, ably assisted by Dr. Murray of the Marine Hospital Service, and in the midst of my exhortation heaven lent her harmony as a hand-organ in the street played that old familiar hymn, "Almost Persuaded," and I feel confident that if Dr. Souchon had been present, I would have made a convert of him also.

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Dr. William Bailey, of Louisville, Ky.—I want to ask permission to extend five minutes more time to Dr. Souchon. As he is the personification of this side of this question, I want to hear further from him.

Dr. C. P. Wilkinson, of New Orleans.—The practical discussion of this question comes home to us when certain things have been accomplished. When the United States authorities acted on the theory that yellow fever was also transmitted by fomites, the city of Havana was thoroughly treated at a great deal of expense. The result was that the average number of deaths from yellow fever at that time was just the same as it had been for the past twenty years. But acting on the theory that the mosquito was the only transmitter of the disease from man to man, that theory was put into application and execution, and yellow fever was wiped out of the Island of Cuba.

Dr. Tom A. Williams, of Houston, Tex.—Before Dr. Souchon speaks again, I would like to point out two facts in which he is absolutely in conflict with the statements that have been made by other observers, and Dr. Souchon will have an opportunity of replying to my criticism, if he sees fit to do so.

The first was with regard to the time of the year during which the experiments of the Havana observers were conducted, that is, in November. But Dr. Ross stated that he himself conducted experiments during the season of maximum prevalence of yellow fever. That would be a reply to the contention of Dr. Souchon as to the time of the year.

With regard to the other point, Dr. Souchon stated that the *Stegomyia fasciata* could not live more than five days without water. That was in direct conflict with the statement made by Dr. Ross, when he said that Dr. Howard, the United States entomologist, stated positively that the *Stegomyia fasciata* was capable of hibernation. There must be some reconciliation between these two conflicting statements. One of them must be right, and it is the duty of the Association to find out which one of them is right and base its finding accordingly. Granted that mosquitoes cannot live more than five days without water (according to the remarks of Dr. Souchon) during that time, then the infection cannot be conveyed by mosquitoes after a period of several months; but, I believe, even if Dr. Souchon is right in that argument, his contention will fall to the ground when we consider the fact that, so far as we know (it may be known to the Marine Hospital Service), the organism itself concerned in the production of yellow fever is unknown. We argue from our knowledge that it is similar in nature to the organism of malaria and to the organism which causes the disease among cattle known as Texas fever, but which is now understood to be and is known as tick fever. I understand, the disease tick fever is capable of being transmitted through the medium of the ova of the pyrosoma. It is capable of residing for long periods in the ova on pasture and infecting the non-immune cattle when they move to that pasture. May not the organism of yellow fever reside in the ovum of the stegomyia and be capable of transmitting the disease to individuals who come in contact with the mature stegomyia hatched from that ovum?

I do not pretend to say anything whatever of the disease itself, because my experience with it is nil. I have only been in this country three months. But I wish to draw the attention of the Association and of the future speakers to these considerations, so that further light may be thrown on these points.

DR. HAMILTON P. JONES, of New Orleans, La.—In 1897 I was in charge of the yellow fever hospital in this city, at the Beauregard School out on Canal street. That school was situated in the midst of a square of ground that had been formerly cultivated as a potato field, and I know that the mosquitoes were prevalent and exceedingly annoying. We had 216 cases of yellow fever there. We had three Sisters of Charity, two of whom did not have the disease. We had eight nurses, none of whom had yellow fever. That hospital, before it was opened, was burnt by the angry population of this city, but within a radius of ten squares there was not a case of yellow fever. Over three hundred people were exposed to the influence of these mosquitoes and to the influence of fomites, and yet not a soul contracted yellow fever from the hospital, to my knowledge. I am still in doubt as to whether fomites or mosquitoes are capable of conveying yellow fever.

Dr. Juan Guiteras, of Havana, Cuba.—I wish to show some photographs which illustrate in a graphic manner the work that is being done in Havana in connection with yellow fever. I shall describe briefly how this work is done, in order that it may be known to you, and it may turn out some day that Dr. Souchon is perhaps moving too slowly in regard to yellow fever. Of the large cities of the world, I believe Havana was the first to have instituted a systematic campaign and prophylactic measures against malaria and yellow fever. We have had a practical and conclusive demonstration made of this doctrine by the United States Army Board, owing to the well directed zeal of the American government in Cuba through its representatives, General Wood and Major Gorgas.

With respect to yellow fever, the city of Havana, under the direction and supervision of Dr. Finlay, now health officer of the island, has kept up and carried into execution practical measures of prophylaxis. The measure of success that has been thereby attained is such that we cannot help but regard with apprehension any evidence of unwillingness to accept this doctrine as the sole basis upon which prophylaxis against yellow fever should rest. We are in a position to specify a method for the prevention of this disease, and we consider time, money and energy wasted in not directing attention to it. It is obligatory, under penalty, for all physicians to report at once all cases of a suspicious character. Let us suppose a case report indicates a suspicion of yellow fever. That report is received by the statistical division of the sanitary office and is at once transmitted to the executive officer. This officer passes it at once to the section on inspection of infectious diseases and to the commission for infectious diseases. If the inspectors confirm the suspicion, they proceed at once to bar the room occupied by the patient against the egress and ingress of mosquitoes. Meanwhile, the case is visited by the members of the commission, who decide as to its nature, and according to their findings prophylactic measures may be continued or not. The prophylactic measures are carried out by a section of the mosquito brigade.

If the patient can be transferred to another room, the latter is disinfected first, that is, the mosquitoes are destroyed, and the doors and windows are provided at once with wire gauze. If the patient cannot be moved, the rooms are protected with thick manila paper to protect him against the smoke during fumigation. If these measures cannot be carried out satisfactorily, the patient is removed to Las Animas Hospital. The destruction of mosquitoes in the room is carried out in the following manner: The chief, so-called, measures the cubic space of the walls; the workmen close the openings with the necessary strips of paper and paste. One door is for an exit, and after the petroleum has been set fire to, we attend to every cubic foot of space; the room is allowed to remain closed for four hours. At the time of fixing up the room, a small window, or a window with a glass pane, is left open, because we find the mosquitoes generally fly to the light, they avoid the smoke, and come to die close to this window, so that they are readily found afterwards and swept up near the window. It is found also that the mosquitoes are very likely to fall at this place if some white paper is laid near the window. At the end of four hours the door is partially opened and the men enter to shake up everything in the room, the heavy clothing, bedclothes, and sweep down the walls and ceilings in order to gather up all the mosquitoes. If some of them are found only intoxicated by the smoke and are not dead, they are burnt at once, and pyrethrum powder is used, otherwise those that are dead are enclosed in boxes and sent to the laboratory to determine what kind of mosquitoes they are, and whether the stegomyia is present or not. Sometimes it is present in different places, sometimes it is not. Besides, the men are ordered to throw petroleum on sanitary surfaces of the house. The inspector also presents a report of the sanitary conditions of the house, with recommendations of what he deems advisable. He visits and investigates the places where the patient may have been infected; he also visits places the patient may have been during the five days previously and the other persons who may have been bitten by the same mosquitoes as the patient himself. He is also requested to make a census of the non-immune inhabitants about the house.

I have here a series of photographs showing some of this work, the disinfection of rooms, the water barrel, etc. Every householder is obliged to keep his water barrel, if he has one, covered with wire gauze. Here is a photograph (exhibiting) showing what we call the yellow fever disinfecting brigade arriving at the house, the distribution of the pyrethrum powder, measurement of the room, the pasting of the walls, cracks and joints between doors and windows and walls, covering them with strips of paper. These two photographs (exhibiting) I wish to dwell on particularly. I have no doubt that if a case of yellow fever was announced in the city of New Orleans or came in on the railroad from Pensacola, there would be great excitement. Quarantine would be so rigid in some places that I doubt whether the case would arrive here, as it might be stopped on the way. I will ask you to contrast that state of affairs with what would prevail in the city of Havana, if we had a case there, a city where yellow fever seems to spread with greater ease than anywhere else. There would be no excitement whatever. We would feel absolutely sure that no harm would come from that case. We would send an ambulance to the wharf to receive the case. The case would be taken off the ship, with all parts of the body well covered with a sheet thrown over the patient, and as soon as he gets into the steam launch he is brought rapidly to the wharf. At the wharf he is received by the ambulance corps. That is shown in this picture (indicating). The ambulance is provided with a mosquito bar. The patient is placed at once in this ambulance under a mosquito bar, is carried quietly through the streets of Havana, and everybody knows from the mosquito bar what is in the ambulance. The patient arrives at Las Animas Hospital. On his arrival, being still under the mosquito bar, he is taken out of the ambulance, his face well covered, and is taken into the hospital. I present here some photographs showing the Las Animas Hospital, with careful screening against the introduction of mosquitoes, double doors, etc. In this hospital all sorts of cases are treated. It is an old Cuban residence, with very large doors, one room communicating with the other. These doors are all closed by wire screens. These doors are never closed unless there is a female patient who wants to be private, but there are very few female patients in this hospital; the majority of them are male patients, and these doors are always kept open except the mosquito bar, and frequently a non-immune is lying in a room next to the one in which a case of yellow fever is being treated.

I wish to call attention to one fact that has not been mentioned, from the point of view of parasitology. Everything points to the fact that in connection with yellow fever we are dealing with a protozoan parasite. From the standpoint of parasitology, we can maintain also as firmly, as upon clinical grounds, that the mosquito is the only means of transmission of yellow fever. It is well known that parasites all live what we may call a miserable life; they are obligatory to a certain mode of living and to a particular host. In the course of evolution, the parasite has accustomed itself to live in that particular manner in a certain host. It has no other way of living. In the course of evolution, the tapeworm, for instance, the tænia, has become habituated to live in man and pork, and at the present time it has no other way of living. It follows a definite cycle of development, both of these animals being essential for this cycle of development, and without these two animals the parasite cannot live. It is the same with trichina; it is the same with the malarial parasite. The malarial parasite has for its cycle of development two animals—man and the mosquito, and outside of that cycle it is impossible for the parasite to live. It has no other way of living. It must go from mosquito to man and from man to the mosquito. It is the same way with the parasite of yellow fever. In the course of evolution, it has become accustomed to this circumscribed, obligatory mode of life, and it has no other, and thus from a scientific point of view, as well as upon clinical grounds, it has been demonstrated that the mosquito is the only means of transmission of yellow fever.

Dr. H. R. Carter, Public Health and Marine Hospital Service, of Baltimore, Md.—I have written about all that I have to say as to the mosquito (stegomyia fasciata) being the only means of conveyance of yellow fever. Those who are sufficiently interested in the subject may read the best knowledge I have regarding the conveyance of yellow fever in my articles.

What Dr. Guiteras has said was really the only thing I had intended to say, and which to me is almost sufficient proof by itself; namely, that it is a biological law, that if a disease be conveyed by an animal host, it is an animal parasite that causes the disease, and that this animal parasite has only the one host, besides man, in which to live. It has no culture medium and no third host that we have ever found in any disease which we have been able to investigate. It has been conveyed in but one way. The burden of proof would then be for the opposite party to show that this disease was in opposition to all apparently kindred diseases, and their number is very considerable. Not simply malaria and tick fever, but many diseases are conveyed to man and many to other animals through animal hosts. They are all animal parasitic diseases, and they all have only two hosts in which they live. But I will say nothing further regarding this phase of the subject.

Dr. Souchon is justified and absolutely right in saying that he wants light on this subject, particularly with reference to the transmission of yellow fever by fomites. But I think we have light—much light in this matter. The discussion centers around the conveyance of yellow fever by fomites, and believing as I do that fomites are absolutely innocuous, I am sure Dr. Souchon will receive light and the rest of us as well. Let me say a few words now as to the innocuousness of fomites. In the first place, to say that fomites do not convey or transmit yellow fever, is a negative statement. It is only negative evidence that I can bring to bear in its proof. I cannot give you the results of experiments as Dr. Reed did for the positive proposition that the mosquito *does* convey yellow fever, and prove it. One observation is sufficient proof that the mosquito *does* transmit this disease. One proof of failure of conveyance of the disease by fomites would not prove that fomites do not convey the disease. It would simply prove that fomites did not convey the disease at that time. It will require a large number—a very large number—of such observations to be convinçing. Like Dr. Souchon, I did not attach much weight to the experiments made by Dr. Reed and Dr. Carroll with regard to fomites, which were made in November in Havana. But it is needless to go into that phase of the subject. Anyone who is familiar with the subject will see at a glance the defect. These experiments, if made by Dr. Guiteras or Major Gorgas, would have been differently conducted. The experiments made by Dr. Ross stand on a different footing. Dr. Reed's fomites came from a place which we have reason to believe was not infected, namely, the Las Animas Hospital. On the other hand, the experiments of Dr. Ross were conducted at a place which is usually infected and during the hot season of the year, consequently their results are more important.

In the conveyance of the disease by fomites, let us, for argument, forget all we have learned from Reed and Finlay and go back to the nomenclature of three years ago. Conveyance of yellow fever by fomites is conceivably possible in two ways, and in but two ways. First, the direct conveyance from fomites to man, as smallpox is. Second, the conveyance from the fomites to the environment and from the environment to the man. I think you see the difference between them clearly and plainly. Every observation I have ever seen recorded has predicated *direct* conveyance from the fomites to man without any intermediary mode of infection of place. Now mark you, for this direct conveyance by fomites we do not require what we call "infectable place." It would take place, no matter where the exposure, no matter what the temperature. Now we have negative evidence that fomites have very little or nothing to do with the conveyance of yellow fever to human beings. And no little of it. For many years—nineteen of them—preceding the summer of 1898 there was no disinfection of baggage on vessels at the New York quarantine station from the ports of Havana, Veracruz, or Santiago, except when cases of yellow fever were on board. It is hard to say how many people passed from these ports for New York during these many years. Up to the same time—for twenty-seven years—there was no disinfection of fomites or of baggage for Spanish ports for the same places. I do not know how many men passed—through these ports for Spain during that time. To estimate it on the basis of the number that passed through my office, when in Havana, over two lines would give, in the *summers* of these years 392,000 people as passing to these places. I will be a little conservative and estimate it at 300,000. In addition to that, all the soldiers with baggage went back to Spain at the end of the war—how many we do not know, but approximately 208,000, came over to Cuba in the last consignment by the Spanish Trans-Atlantic line. Two hundred thousand men with their baggage—think of it! I do not count the baggage of the servants and the officers. Roughly speaking then, we have here the baggage of more than half a million of people going from Havana and Santiago alone, without counting those from Veracruz, to New York and Spain. It can be said that much of this baggage was not infected; that much of this baggage, which belonged to the non-immunes, may not have been exposed. But with that amount of baggage to experiment on, it is reasonable to suppose that some of it must have come from houses in which yellow fever *was being contracted;* you know that a house in which yellow fever *exists* is not necessarily infected. If yellow fever is *contracted* there, it is infected. Of 500,000 people, some of their baggage must have come from houses in which yellow fever was being contracted, and under the old nomenclature it would be looked upon as "infected baggage." Baggage going to Spain is more likely to be infected; that going to New York is usually in good condition. All of that baggage presumably passed through the custom houses and was inspected by custom house officers, while all of it went to hotels in New York, was handled, much of it, by the non-immune chambermaids in the presence of non-immune people, and *it is not possible* but that such baggage, if it generally was capable of conveying yellow fever, should have introduced it in very many instances into New York and Spain. Yet there is no record during that time of any case of yellow fever in Spain; there is no record of any case of yellow fever in New York. I will admit that a few cases might have occurred and not been reported. But if that baggage could convey infection, enough cases must have occurred to have compelled report and many reports. I leave out the fact of Spain having been at times "infectable territory." It would be a good point, but it might not be fair because while Spain has been infectable territory one year, it does not imply that it is capable of becoming infected another year. Yellow fever spreads this year in one place, but does not do so the next.

Does this not show that baggage from Havana, from Veracruz, or from Santiago de Cuba does not convey infection to people in New York or in Spain? We know there is no peculiarity in the people in those places, and I think it is a fair deduction to say that this baggage will not directly convey infection to any people after a passage equal to that to New York or to Spain. I do not believe that the time element is an important factor, but neverthelesss it comes in and logically we cannot very well throw it out.

For the second possibility of transmission of infection by fomites, I cannot give you anything like as good reasons against it.

As to infection of environment, I will simply say that no writer has ever claimed infection from environment by fomites, by clothing, etc., and it seems needless to attack what has not been advanced. When the cleaning of the city of Havana from yellow fever was accomplished, if fomites were capable of conveying infection by infecting environment, we should have had more cases of yellow fever. The results here (in Havana) contradict that mode of infection. Dr. Finlay has been continuing experiments on fomites for us all this year. All last year Veracruz had an epidemic of yellow fever, and he has taken precautions against the introduction of people into the city of Havana suffering from that disease. He has said that he has taken precautions against the introduction of the stegomyia, the host that carries the disease. But he has been admitting now the clothing and baggage from Veracruz without disinfection, and so far, he has no ill results.

I am not finding fault with those who require further light on this subject, for each man must judge for himself as to whether the argument is convincing or not. An argument that may be convincing to me may not be convincing to you. If the evidence already advanced is not enough, and for me it is, I am sure that, if Dr. Finlay keeps up his present quarantine, and the Spanish immigration to Havana continues, from Havana will be furnished additional evidence that yellow fever is not transmitted, even indirectly by infecting the environment, in a community of non-immune people and in territory eminently "infectable"; and it should be light enough for all of us.

Dr. Quitman Kohnke, of New Orleans, La.—I do not propose to enter into a discussion of the mosquito theory of the transmission of yellow fever; I have accepted it long ago. Dr. Souchon seems to be a defender of the fomites theory, and regards as of higher value the phenomena of nature, than those produced, or brought about, by the agency of man. The observations and experiments carried on by man are not accurately comparable with the conditions obtaining in nature; but it is our experience that, similar conditions obtaining in nature, when recorded by man, are certainly more likely to be erroneously recorded and transmitted than experiments, such as those undertaken under conditions obtaining in Cuba at the time the experiments of Reed, Carroll, and others were made. That the stegomyia may hibernate throughout the winter seems to be evidenced by our experience here. For instance, in 1897 the board of health, then the state board of health, having charge of city affairs, disinfected yellow fever-infected houses with formaldehyd gas. In 1898 instances were reported where non-immunes, living in rooms that had been disinfected for yellow fever by formaldehyd gas, contracted yellow fever; supposedly from infection in those rooms. In 1898 sulphur disinfection was employed, and it is believed that no infection was carried over from 1898 to 1899, although there occurred yellow fever in the city in 1899.

The practical application of what has been said with regard to the destruction of mosquitoes, and the germicidal advantages of formaldehyd, as compared with sulphur, seem to indicate that, formaldehyd disinfection was ineffectual and sulphur disinfection was effectual, because sulphur killed the mosquitoes, while formaldehyd did not. The board of health of the city of New Orleans is now prepared to treat all cases on this theory that are even remotely suspected of being yellow fever. Last summer, and the summer before, suspicious cases are said to have occurred. There are some malarial fevers every summer about which someone has a modicum of doubt. These must be regarded in a measure, and for a time, as suspicious; although not suspicious in the sense required for reporting cases. If there is any suspicion in the mind of anybody that any case may be yellow fever, the board of health disinfects, according to the mosquito theory of the transmission of the disease; several cases having been so treated before a diagnosis of malaria could be made. I desire to make the position of the health authorities of the city very clear. We accept the mosquito law in regard to yellow fever, and have acted upon it, in so far as we have been able to do so. (Here Dr. Kohnke read some extracts from the annual report of the city board of health of New Orleans, setting forth clearly the position of the board in regard to the disinfection of fruit vessels from infected ports, and stating that the quarantine authorities had been appealed to for the re-establishing of this precaution, which had been withdrawn a few years ago.)

The practical application of what everybody has said this evening, including the remarks of Dr. Souchon in defending the fomites theory, would, in my opinion, be this: That quarantine should be most strenuous against that which has been proven infectious; even at the expense, if necessary, of those measures employed against that, which is only suspected. Where measures against both fomites and mosquitoes can be operated no valid excuse can be urged for neglecting the mosquitoes. Certainly, fomites cannot be considered as proven to be infectious; they are only suspected of being infectious. The mosquitoes are the transmitters of yellow fever. Here is a known quantity, against which a strong effort should be made; even at the expense of those measures directed against the suspected, but not proven method. If, however, measures against both the known and suspected can be carried out without disturbance to either, then, surely, both measures should be carried out, and I am glad to know that the quarantine authorities recognize the necessity of disinfection against the known transmitters of yellow fever, the mosquitoes. It should be easy, it seems to me, to prevent the entrance into any city of mosquitoes, either by vessel or by railroad. The measures advocated this evening would, if employed and vigorously applied, prevent infected mosquitoes reaching any place where non-immunes reside. The stegomyia have been destroyed in Havana largely because the men who tried to do it, and did it (if not completely, practically), had behind them the strong arm of a determined government and its treasury. One of the speakers this evening referred to the fact that $25,000 per month was the expense for removing the stegomyia from Havana. It would be impossible to employ the Havana measures in any large American city, such as this. Therefore, the more strenuous should be those against the reception of infection. These measures do not require such an enormous expense. Indeed, they are a part of the measures now employed against infection from fomites, and require only to be properly directed against mosquitoes. It has seemed to me that the trend of quarantine authorities must in the future be towards withdrawing disinfection against fomites, and enforcing more vigorously disinfection against mosquito infection.

It would, perhaps, be impossible in one evening or two evenings to go over the ground fully, but I believe this discussion will excite debate elsewhere, and that, eventually, the mosquito law will be applied where it is most needed—to the quarantine service.

Dr. Souchon (closing the discussion).—I will state for the information of Dr. Kohnke, that Dr. Ross in his paper said that it cost $7,000 to kill mosquitoes in the city of Havana. I fully believe in the disinfection of mosquito-infected places, and of baggage until we obtain additional light on this subject. The mosquitoes were not destroyed at once by the State Board of Health. It is being proven by actual experiments that the mosquitoes on the vessels that have been referred to did not carry yellow fever, and the reports received yesterday from Dr. Howard, of Washington show that of all the mosquitoes, according to the report of the medical inspectors, during the whole season, over two hundred, there were but 3 per cent. of stegomyia on board those vessels, and they were not thought to be infected.

In reference to the remarks of Dr. Carter, that baggage sent from Veracruz, and from Havana to New York and Spain, did not contain yellow fever, permit me to say, that if he had sent that baggage to Mobile or to New Orleans it might have been a horse of another color.

In reply to Dr. Carter and Dr. Guiteras as to the transmission of yellow fever from man to mosquito and from mosquito to man, the great trouble is we do not know what kind of a germ it is as yet. If it is an animal germ, yes; but if it is a bacterium, it may be transmitted in various ways.

As to the remarks of Dr. Wilkinson in regard to the absence of yellow fever in Havana, I contend that if there is no yellow fever in Havana at present, it is due to the American quarantine that kept it out. Let the cases in now, and they will see what they can do in a short time.

As to the stegomyia living only five days without water, it does not live longer than five days in the *summer* months. During winter months it may hibernate or live all the time.

Dr. F. J. Mayer offered the following resolution:

Resolved, That it is the sense of this Association that all Gulf and South Atlantic ports of the United States without adequate drainage and sewerage systems, and with unreclaimed swamps, and other breeding places of mosquitoes adjacent thereto, should in the interest of health and commerce, make haste to rectify the existent evils, but should make haste *slowly* in leaving the beaten paths of safety by any radical modifications of the existent quarantine restrictions against yellow fever ports in order to make them conform with the new doctrine of mosquito infection.

That while admitting the evidence seems to point to the mosquito as the intermediate host, there are questions concerning the importation of infected mosquitoes that require further study, investigation, and experimentation before any radical modification of existing quarantine practice can be with absolute safety recommended. (Referred to the Executive Committee without discussion.)

On motion of Dr. McCormack, the Association adjourned until 10 A. M. Thursday.

DECEMBER 11, 1902—THIRD DAY—MORNING SESSION.

The Association was called to order at 10 A. M. by the President.

Dr. C. P. Wilkinson.—Yesterday afternoon the Committee of Arrangements noticed that a number of the members availed themselves of souvenirs of the trip to the Kenilworth plantation. I am instructed by the Committee of Arrangements to hand to the President of this Association a gavel made of orange wood, as a souvenir of the meeting.

The President.—To Dr. Wilkinson and other members of the local Committee of Arrangements: I desire to express my personal gratification for this handsome and beautiful souvenir, one which will, with its memory, give me a fond recollection of New Orleans and its inhabitants, and especially the Committee of Arrangements. I trust that I shall wield it with fairness and impartiality during the remainder of the sessions. I thank you. (Applause.)

THE PRESIDENT.—The next order is the report of the Executive Committee by the Secretary.

THE SECRETARY.—The following resolutions submitted to the Executive Committee, which were offered by Dr. Wilkinson, are reported back with the recommendation that they be adopted.

WHEREAS, At the last meeting of the American Public Health Association, held in Buffalo, N. Y., in September, 1901, Dr. Albert L. Gihon, an ex-President of this Association, was appointed Chairman of a committee with power to select his associates, with instructions to prepare suitable resolutions to be submitted to this Association at its meeting in 1902, in memory of Henry B. Horlbeck, deceased, an ex-President of this Association; and,

WHEREAS, Albert L. Gihon was himself stricken with death before said resolutions had been prepared; and,

WHEREAS, Suitable mention has been made of the death of Albert L. Gihon, and suitable resolutions in his memory have been adopted and no suitable resolutions respecting the death of Henry B. Horlbeck have been presented to this Association; therefore, be it

Resolved, By the American Public Health Association, that in the death of Henry B. Horlbeck the Association mourns the loss of an esteemed officer and a valuable member, and his associates mourn the loss of an active, zealous and efficient co-worker and a dear and genial friend.

THE PRESIDENT.—You have heard the recommendation of the Executive Committee in regard to the resolutions concerning Dr. Gihon and Dr. Horlbeck. What will you do with the report?

It was moved and seconded that the recommendation of the Executive Committee be concurred in. Carried.

THE SECRETARY.—The resolution referred to the Executive Committee, which came from the State of Michigan, has been altered, and it now reads as follows, and the committee recommends it to the Association for adoption:

Resolved, That the American Public Health Association cordially approves of all efforts of the Michigan State Medical Society to promote the establishment of a state sanatorium for cases of tuberculosis, and trusts that suitable legislation may be enacted for this commendable purpose.

THE PRESIDENT.—You have heard the reading of this resolution; the Executive Committee recommends its passage.

On motion, the recommendation of the Executive Committee was concurred in.

THE SECRETARY.—With reference to the resolution which was referred to the Executive Committee relative to the appointment of a medical officer on the commission in connection with the construction of the Isthmian Canal, the Executive Committee considers it inexpedient to adopt the resolution at this time.

On motion, the recommendation of the Executive Committee was endorsed.

THE SECRETARY.—The Executive Committee has considered the resolution offered by Dr. Mayer relative to the subject of yellow fever, and it reports it back to the Association, recommending that it be declared inexpedient to pass the resolution at this time.

DR. JOHN L. PORTER, of New Orleans.—Would it be possible for a representative of the Executive Committee to state the reasons why it deems it inexpedient to pass the resolution offered by Dr. Mayer. I would like to know the reason for this action on part of the Executive Committee.

THE SECRETARY.—The Secretary is not prepared to go into details as to what was done by the Executive Committee in regard to this resolution.

DR. PORTER.—I would like to have the original resolution re-read.

The Secretary then re-read the resolution offered by Dr. Mayer.

DR. JUAN GUITERAS.—I move that the recommendation of the Executive Committee be sustained. Seconded by Dr. Finlay, and carried.

THE SECRETARY.—The following amendment to the constitution has been offered:

Section IX of the Constitution is amended by adding thereto and following the words "Of the ex-presidents of the Association," the following words: "The Chairman and Secretary of the laboratory section and the retiring Chairman of that section." (To lie over one year.)

Dr. John C. Shrader, of Iowa City, Ia., moved that the President appoint delegates to the American Congress of Tuberculosis, to be held in St. Louis, Mo., in 1904. Carried.

The President.—If there are no other committees ready to report at this time, we will listen to the report of the Treasurer.

Dr. Frank W. Wright, of New Haven, Conn., then presented his report as Treasurer of the Association, as follows:

REPORT OF THE TREASURER.

FRANK W. WRIGHT, TREASURER, IN ACCOUNT WITH THE AMERICAN PUBLIC HEALTH ASSOCIATION.

1901.	*Receipts.*	Dr.
October 4—	To cash from Henry D. Holton	$1,197 83
	To cash from dues of members for 1899	10 00
	To cash from dues of members for 1900	110 00
	To cash from dues of members for 1901	1,090 00
	To cash from dues of members for 1902	45 00
	To cash from sale of Transactions, essays, etc	179 70
	Total	$2,632 53

1901.	*Disbursements.*	Order No.	Cr.
October 26—	By cash paid A. C. Anderson	122	$ 10 00
	By cash paid Herbert D. Pease	123	9 85
	By cash paid C. O. Probst	124	62 95
	By cash paid George C. Whipple	125	11 50
	By cash paid Henry D. Holton	126	91 25
December 20—	By cash paid William Whitford	127	158 60
1902.			
May 16—	By cash paid Berlin Printing Co	128	1,112 03
July 28—	By cash paid U. S. Express Co	129	8 37
	By cash paid Berlin Printing Co	130	26 45
	By cash paid American Express Co	131	101 82
September 29—	By cash paid C. O. Probst	132	253 00
December 1—	By cash paid Frank W. Wright	133 and 134	65 75
	By cash paid C. O. Probst	135	59 37
	By cash paid Berlin Printing Co	136	82 00
December 2—	Cash on hand		579 59
	Total		$2,632 53

The President.—The Treasurer's report will be referred to an auditing committee consisting of Drs. J. N. McCormack and C. S. Caverly.

The President.—The Chair desires to depart from the custom which has existed for many years, namely, that the retiring President shall appoint the committees for the succeeding year at his pleasure.

That is usually done after the Association has adjourned for that year, so that the members of the various committees are not informed of their appointments for a considerable period. Furthermore, there is no opportunity for them to consult one another. It has been deemed wise this year to appoint the various committees as early as possible, so that the members of committees can get together and confer with reference to the work which they are expected to do during the coming year. So I take pleasure in announcing the committees at this time. (For list of committees, see appendix.)

The Secretary called the roll of members of the Advisory Council and members were appointed to fill vacancies.

Dr. Felix Formento, of New Orleans, La., read a paper entitled "Tuberculosis and Agricultural Colonies." (See page 300.)

Dr. Tom A. Williams, of Houston, Tex.—Mr. President: I have listened to this paper with the very greatest attention because it embodies an idea which I believe is essential toward the solution of the tuberculosis problem. I did not catch all of the words of the paper. As far as I understood it, this was the idea, that in the south of France a small municipality had established a colony where consumptives, after being cured in a sanatorium, could permanently reside, engaged in agricultural pursuits, so that the certainty of recurrence of the disease in a great number of cases, did they return to the cities, would thereby be obviated. That is the inevitable ending of a large number of so-called cured subjects of pulmonary tuberculosis.

If I remember rightly the statistics of the Nordrach Institution are somewhat as follows: Dr. Walther, perhaps the most eminent exponent of the sanatorium treatment of tuberculosis, returns 90 per cent. of his patients in a condition of arrest or cured. He receives only patients in the first stages of the disease, but the demand has become so great that he is compelled to take a large number of cases in the secondary stage of the disease. Of this 90 per cent., 45 per cent. are absolutely cured. They are enabled to return to their former occupations and are able to maintain their health for a number of years under full work, and by that I mean arduous work, as conducted in our cities, very often amidst the most unhygienic surroundings. The number of recurrences among these 45 per cent. is small indeed. Thirty-seven per cent. of the patients return to work under the same conditions, but feel a diminution of capacity and a lessened power of resistance to hardships, exposure, long hours of work, privation, etc. That is to say, their constitution has been damaged by the struggle with the disease, although they may no longer show the effects of the tuberculosis. The other 8 per cent. can only live under the most favorable conditions. Their constitutions are not established to the extent necessary to engage in the ordinary work of the world. These results are very encouraging.

This is merely what I wish to point out: We already have accommodations in this country and in a great many countries for wealthy people. A very wealthy man may be willing to go to a sanatorium and pay the sum of from fifty to one hundred dollars per week for treatment; time is no object to him if he can be cured. He has spent, perhaps, considerable time in his life in traveling between different health resorts with the hope of being cured. The poor man will have provided for him, I trust, state sanatoria in which a certain number of cases will be cured as soon as the medical profession appreciate the enormous importance of early diagnosis which is so essential if we wish to stamp out tuberculosis. But there is no provision in this country, nor is there any indication of provision for that large army of men, the workers of the nation, the men who are not able to pay one hundred dollars per week for a period of six or eight months and have to live at health resorts at great expense. The men who constitute the greatness of your nation, great railroad men, great lawyers, great business men who in youth are not wealthy, who have to make their own way, and who often fall in the struggle owing to the unhygienic surroundings in which they have to live, are placed at a great disadvantage. This should not be so; but it is so. It is our duty to provide means whereby these men, when they fall in the struggle, may be restored to their positions in life.

Let the agricultural colony be established at a distance not too great from the sanatorium. Let these men live in suitable dwellings, constructed with a view to an open air life (which is necessary during the whole twenty-four hours) on this agricultural holding. Let them there cultivate each (or in coöperation) his holding. Let there be a separate holding or one in coöperation, under the direction of the whole population. Thereby these men will support themselves or partially so. I think, gentlemen, that can and will be done, and I think it is a solution of the question for that class of people. At the end of one or two years these men will be so built up in constitution that they will then be able to return either to their former positions in the cities of the world, or will make up their minds to adopt agricultural pursuits, which will be a great advantage on account of them being men of intelligence and education. In speaking of men I wish to include women also. But the most important of all the functions which such an institution will subserve will be the fact, I believe, that 70 per cent. of these men will return to the general population. Each of these individuals will be a source of light, will spread the information they have obtained.

Dr. Samuel H. Durgin, of Boston, Mass., presented the report of the Committee on Dangers to the Public Health from Illuminating Gas. (See page 130.)

Dr. Henry Mitchell, of Asbury Park, N. J., offered a resolution in regard to the danger to the public health by illuminating gas. (Referred to the Executive Committee.)

Dr. F. F. Wesbrook, of Minneapolis, Minn., presented the following report:

The Committee on Transportation of Diseased Tissues by Mail was given power to act and has matters in such condition that final recommendations can be made to the postal authorities represented in the American Public Health Association. Your committee, therefore, begs leave to report that there seems to be no necessity for its continuance.

On motion, the report was adopted and the committee discharged.

The report of the Committee on The Relative Immunizing Value of Human and Bovine Vaccine Virus was read by Dr. C. P. Wilkinson, in the absence of the Chairman, Dr. Peter H. Bryce, of Toronto, Ontario. (See page 146.)

Dr. Jésus Gonzalez Ureña, of Mexico, read a paper entitled "Only Asepsis Necessary in Vaccine." (See page 162.)

Dr. Tom A. Williams of Houston, Tex.—I beg to offer the following resolution:

Resolved, To prevent the serious loss of valuable lives from pulmonary tuberculosis among those who return to the general population after the arrest of their disease in sanatoria, this Association desires to urge upon those about to establish such sanatoria the necessity of providing means in the nature of agricultural colonies for so establishing the health of patients, at an expense within their means, as to enable them to return to the industrial population without that risk of a recurrence of their disease now almost inevitable. (Referred to the Executive Committee.)

Dr. F. J. Mayer offered the following preambles and resolution:

Whereas, Tuberculosis, "the great white plague," continues to carry off one in seven of the people of the civilized world, mainly owing to the *ignorance of the masses* of the fact that the dessicated sputum of tuberculous victims is the chief vehicle of transmission;

WHEREAS, In despite of the fact that over 100 years have elapsed since the genius of Jenner gave to the world a prophylactic against variola (smallpox), and yet every year records recrudescences of this disease owing to the *ignorance of the masses* as to the nature and prevention of the disease, and an unfounded prejudice against vaccination and revaccination;

WHEREAS, Typhoid fever is growing progressively more frequent in certain rural sections of the country in a direct ratio to the increase in population, again owing to the *ignorance of the masses* as to the cause and prevention;

WHEREAS, Anthrax is endemic in certain sections of the Union periodically threatening the live stock industry of those sections, mainly owing to the *ignorance of the masses* as to the proper sanitary disposition of charbonous carcasses; be it

Resolved, That Bowditch's plea for more perfect organization "from the highest place in the national council to the smallest village board of health, for the purpose of studying contagious and infectious diseases," should meet our approval; that the highest duty of the state is to afford public instruction in the fundamental principles of hygiene, and the true nature, cause, and prevention of contagious and infectious diseases. (Referred to the Executive Committee.)

On motion, the Association adjourned until 3 P. M.

THIRD DAY—AFTERNOON SESSION.

The Association reassembled at 3:25 P. M., and was called to order by Dr. Felix Formento, in the absence of the President.

Dr. Charles A. Lindsley, of New Haven, Conn., gave a verbal report of the Committee to Investigate the Canteen System of the United States Army. Dr. Lindsley said: Our report is a very brief one. I have to say, that the members of the committee appointed by the Association never had a personal conference, but I have had correspondence with all of the members of the committee, and when we began to investigate this subject we found ourselves embarrassed by and beset with many difficulties. We are not empowered or vested with any authority to summon witnesses, and examine and cross-examine them as to the information they may have had from personal observation; but we felt we must depend, entirely upon the official reports in the war department in reference to the condition of the army before, during, and subsequent to the canteen system. This is a matter, of course, in which the committee as a whole could not take much part, and is as well accomplished by one or two members of the committee as by larger numbers, as far as the United States is concerned. We knew that there were in the community some of our most estimable and reputable people, some of our best citizens, who recognize no middle ground between total abstinence and intemperance, and the word canteen for all that class of people settled the whole question. It was decided without hearing and without a trial. Your committee did not feel that that should be the ground upon which they should act; that they should rather take the scriptural injunction to "be temperate in all things," and that we were to consider rather whether the canteen system would promote temperance or intemperance; that that was the real question, whether our army would be in a sanitary condition by the agency of the canteen system, and in a better sanitary condition, than it is at present. To do that, we should have to refer to the reports of officers in regard to alcoholism, insanity, court-martials, venereal disease, and all the other vices that are found not only in the army, but elsewhere; also to know whether the canteen system has had any influence upon the prevalence of these vices, or whether it has diminished them. In order to get at that question, it was the general opinion of the committee that the time was too brief to allow of such a comparison and to justify us in coming to any fair and satisfactory conclusion. We therefore concluded to ask the Association to continue the committee.

At a meeting of the Executive Committee, held yesterday morning, the subject was brought up, and it was suggested that it be recommended to the Association that the committee be enlarged, and that it include not only the United States, but also the provinces of Canada and Mexico. In Canada, and I do not know but what it is true of Mexico, the canteen system in some form is now in existence, and the experience of those countries would help the committee in coming to a conclusion. That recommendation will, I suppose, be acted upon by this committee, and that the committee will be continued and enlarged by the addition of members from those countries. That is all I have to communicate at this time.

The President.—You have heard the report of the Chairman of that committee. What is your pleasure?

Dr. Souchon.—I move that the committee be continued, and that the report be adopted. Seconded and carried.

The Secretary.—I have just received a letter from Professor S. H. Woodbridge, of Boston, Mass., who is Chairman of the Committee on Car and Steamboat Sanitation. He is unable to be present, but he has sent a rather interesting communication which might be read at this time.

THE PRÉSIDENT.—You may read it, Mr. Secretary.

The Secretary read the following letter from Professor Woodbridge.

I regret my inability to prepare the report I had hoped to be able to send, nothwithstanding my handicap in the matter of health heretofore reported to you.

I desire to state that I have had printed the recommendations relating to car sanitation adopted at the last meeting of the Health Association and have sent copies to all the leading railroads and to the leading publications, secular and medical, that I am in receipt of letters from several railroads and boards of health, all of which indicate an active interest in the subject, and copies of some of the articles relating to the matter which have appeared within the last month have also been sent to me. I have also been in touch with men of influence, including Congressmen at Washington, with reference to the enactment of a law under the interstate commerce act which shall prohibit the use of the common drinking cup by passengers known to be ill with infectious disease and I find the sentiment in favor of the passage of such a law encouraging.

In consultation with some of the officers of our leading railroads I find a cordial disposition towards aiding in this branch of sanitary work by the placing of a notice near water tanks in cars, advising the use of individual cups, and I think that any recommendation to this end which the American Public Health Association would adopt would be posted by railroad managements to the great protection of the traveling community.

I have found that porters of sleeping-cars have been retained in the service when they have been known to be so greatly reduced in health and strength by consumption as to be relieved from a considerable part of their duty. I should suppose that a law requiring a certificate of good health on the part of porters would be in order.

My attention has recently been called to the freedom with which consumptives and others afflicted with infectious disease expectorate and use the drinking cups, etc., intended for common use, and very recently I witnessed on a sleeping-car a case of this sort. A man in the advanced stages of consumption occupied a berth across the aisle from my own. I was kept awake the larger part of the night by his labored and painful coughing and his exhaustive efforts to raise phlegm. His sputtering expectorations were frequent; he used the sleeping-car cuspidor, spattering bed clothing, curtains and carpet liberally. When dressed in the morning his attendant brought water to him at least three times within an hour from the tank in the common drinking cup, into which his bushy moustache penetrated nearly to the full depth. Later, other passengers, including children, took water from the same glass.

Experiences of this sort on my own part and similar experiences reported to me by others have compelled me to seek a remedy through immediate legislation if it can be had, and after conferences with parties in Washington, I believe such legislation may be secured, and if not at the present session of Congress through a well-directed effort with the next Congress.

THE PRESIDENT.—You have heard the communication by Professor Woodbridge. What will you do with it?

DR. LINDSLEY.—I believe we have a Committee on Legislation now, and would it not be well to refer it to that committee?

THE PRESIDENT.—We have such a committee, and if there is no objection it will be so referred. It is so referred.

DR. TOM A. WILLIAMS, of Houston, Tex.—I wish to make a suggestion. I am very sorry the attendance is so small. I do not think the American Public Health Association should allow the exceedingly important matter of sanitation of railway cars to pass without further notice or that it be held in further abeyance. The committee, according to what we have just heard, issued a circular to railroad companies, and that circular met with appreciation apparently, and I have no doubt in some cases action was taken. I believe this opportunity should not pass without reenforcing the contentions of that committee, and that it be regarded in some manner, if it is at all possible. As the Chairman of the committee is not present, I think it would be well for this Association to further emphasize the points brought before the railroad companies by that committee. I do not know whether there is any constitutional means of placing the position of the Association before the public other than through the committee. I do not know whether it would be in order to make a motion to that effect. I should like to place before the committee a resolution embodying such suggestions as I have indicated.

THE PRESIDENT.—You may offer such a resolution now.

DR. WILLIAMS.—I desire to offer the following resolution:

Resolved, That the American Public Health Association wishes to reaffirm in the strongest possible manner the findings of the Committee on Car Sanitation, and pending the finding of the Committee on Legislation, wishes again to draw the attention of the railroad companies to the findings. (Referred to the Executive Committee.)

THE PRESIDENT.—As we are short of papers, I am disposed to entertain a motion to give any member a chance to discuss some of the papers or subjects that have been brought before the Association. (The Association took a recess of five minutes.)

On reassembling, Dr. Henry P. Walcott, of Cambridge, Mass., said: Professor Sedgwick, who has had a large and unusual experience in the investigation of various typhoid fever epidemics in Massachusetts, has prepared a paper in conjunction with Mr. Winslow. I have this paper. Professor Sedgwick expected to be here and to read it himself. Unfortunately, he was detained at home. I am going to ask my friend, Mr. Prescott, to read it. I may say, that the conclusions embodied in this paper are essentially those arrived at by the State Board of Health of Massachusetts.

Mr. Samuel C. Prescott, of Boston, Mass., then read the joint paper of Professor W. T. Sedgwick and Mr. C.-E. A. Winslow, of Boston, Mass., entitled "The Relative Importance of Public Water Supplies in the Causation Typhoid Fever." (See page 288.)

DR. W. C. WOODWARD, of Washington, D. C.—Mr. President: With reference to the increases and decreases of typhoid fever in communities, following the rises and falls of temperature, I think we must agree with what has been said by the essayists. I think, however, that there are two possible explanations of the increase in the prevalence of typhoid fever immediately after warm weather, in addition to the explanation offered by the writers of this paper. In the first place warm weather, particularly the hot weather of summer, increases the amount of water that is imbibed. It increases very largely the amount of unboiled water, of cold water, that is taken into the system. Other things being equal, therefore, it increases the chances of infection.

Warm weather is usually accompanied by another condition that favors, in my opinion, the spread of typhoid fever, and that is by low water in streams and wells. It is not the spring freshet, neither is it the flood of the fall rains that increases the danger. These, it is true, make the water muddy, unsightly, and, to a certain extent, unpalatable; they may carry into the water a certain number of typhoid fever bacilli that have accumulated on the surface of the earth about poorly drained farm-houses and poorly drained settlements generally; but they certainly carry into the water supply volumes of water that are proportionately much in excess of the increase in the dangerous bacteria. Therefore, heavy rainfalls, etc., while introducing extra bacilli into our drinking water, instead of increasing the danger tend to swell the streams and to increase the volume of water in our wells—in other words, to dilute whatever infectious matter may be present—and in that way they diminish the chance of infection. But during the summer season, when streams are low, the drainage which finds its way into them is to a greater extent that of farm-houses and of small communities, and in that drainage we find the greatest number of dangerous organisms. Therefore, during the stage of low water, during the stages when the water is clear and palatable, we have a most concentrated form of typhoid-infected water, and that we have found in Washington is the dangerous water. Our typhoid fever death-rate is in an inverse ratio to the number of bacteria found in the water, because while the floods carry in large numbers of bacteria generally, while they carry in a large amount of mud, they do not carry in a large amount of dangerous bacteria, and they dilute the dangerous element that is already in the water.

Dr. Tom A. Williams, of Houston, Tex.—The paper just read contributes to our knowledge of typhoid fever information which, I believe, will be regarded in the future as very important. I wish to contribute certain facts to the discussion from my experience in South Africa. I might say, in the first place, that I agree with the conclusions of the paper, and while I do not exactly disagree with the remarks of the last speaker, I wish to point out what happened in South Africa, and to mention a factor which is of an opposite nature to that mentioned by Dr. Woodward with regard to the character of the water in streams. In South Africa we found enteric fever, as we call it, increased *pari passu* with the increase of temperature. The fact that increase of temperature is accompanied by dryness of small streams in this country is in South Africa shown not to be the cause of the disease, for the reason that in that portion of the country over which my observations extended we have rains in hot weather. During the winter our streams are perfectly dry. Therefore, the streams are flooded at the time enteric fever is most abundant, while, at the same time, the temperature is highest. But we do not regard the disease in South Africa as being water-borne. It occurs in sporadic cases, on scattered farms, and the chief water consumed is rainwater, stored in iron tanks around the houses.

We regard the disease as being spread by Kaffir laborers returning from the gold mines in Johannesburg in a condition of ill health. As soon as a Kaffir feels ill he leaves the mines and goes home. I have examined postmortem or have taken smears of scores of cases in that condition, and have found a large number suffered from enteric fever. I was in charge of one of the refugee camps in which enteric fever was endemic. There were four thousand people in the camp, and we never had more than thirty cases of typhoid fever. Had the epidemic been due to drinking water of a running steram, it would have been of a much more extensive character. We must look upon the cases as directly communicated in the manner mentioned by the authors of this paper, namely, from the dejecta, without the intervention of water, by direct access to the food possibly of typhoid excreta, but not from the water supply itself, the reason being that this epidemic affected so few people that it was inconceivable it might have been derived from the common water supply which everyone drank. The epidemic that broke out among the troops in another district was of a different character, due to the water of a particular river. Some ten thousand were affected at the same time. This river, it is said, was polluted by the Boer army, who had cases of enteric fever among them.

Dr. Arthur R. Reynolds, of Chicago.—Like all the contributions of Professor Sedgwick, this paper is a valuable one. I simply wish to emphasize the necessity for immediate disinfection of the discharges of typhoid fever patients. If this were always faithfully done, I feel that it would greatly reduce the death-rate from typhoid fever all over the country, if it would not wipe it out altogether.

Dr. L. P. Jones, of Greenwich, Conn.—I wish to corrroborate what has been said in reference to this subject and to make a suggestion in regard to the disinfection of stools. For the last year I have introduced a new plan in disinfecting the stools. I place milk of lime with the stools, and in addition to that boil the stools, so that I know the stools are thoroughly disinfected.

Dr. E. L. McGehee, of New Orleans, La.—I rise to express my appreciation of the paper that was read, and to endorse what has been said by the preceding speakers. I do not think, sir, thorough disinfection of stools will ever be carried out until the millennium has taken place. We are not able to watch stools and have them all boiled. They should be disinfected by chemical disinfectants. I believe the stools are a source of typhoid infection, and that the disease will still be spread from that source and indirectly through the water supply. I have here a resolution which I desire to offer now, if I am in order:

Whereas, Several diseases are conveyed by the medium of drinking water; and

Whereas, Legislative enactments limiting the impurities in milk have lessened the dangers from this source; therefore, be it

Resolved, That the American Public Health Association recommends that laws be passed by each state and province compelling hotels and public carriers (railroads and steam vessels) to furnish pure drinking water to patrons and employes. (Referred to the Executive Committee.)

Dr. J. N. Hurty, of Indianapolis, Ind., read the report of the Committee on Sanitary Aid Societies, in the absence of the Chairman, Dr. George W. Goler, of Rochester, N. Y. (See page 165.)

DISCUSSION.

Dr. F. J. Mayer, of Louisiana.—Mr. President: I desire to express my appreciation of this valuable paper. It strikes the keynote of what should be the battle-cry of this Association—public instruction of the masses in hygiene, and the true nature, cause, and prevention of contagious and infectious diseases. It affords me great gratification to say that the State of Louisiana took the initiative in that direction by holding popular institutes of hygiene, with illustrated lectures, and object-lessons. Through the courtesy of the State Department of Agriculture, we have been enabled during the past four years to deliver a series of lectures on hygienic subjects before the farmers' institutes. Our State Legislature, in 1900, by a very decisive vote in the Lower House, and a still more decisive vote in the Senate, passed a law to create a commissioner of hygiene, whose function would be purely educational, and to carry into operation a plan of education as set forth by our State Sanitary Association, in 1897. It was vetoed by the governor, for the reason that the state at that particular time was not able to stand the beggarly expense of an annual appropriation of $4,000. It was passed a second time two years later by the still more decisive vote of 63 to 17, in the Lower House, with 18 of its pro-antagonists absent. In the Senate there was only one dissenting voice. It was again vetoed by the governor on the grounds set forth in his original veto—grounds to a sanitarian a veritable *reductio ad absurdum.* I think it was the immortal Lincoln who said: "With public sentiment nothing can fail; without public sentiment, nothing can succeed. Consequently, he who moulds public sentiment goes deeper than he who enacts statutes or pronounces decisions. He makes statutes and decisions possible of enactment," and before this body can carry into execution the noble work upon which it has embarked, it must educate the masses, who stolidly stand in the way of sanitary progress; and unfortunately it is not only the lower strata of society who impede the efforts of sanitarians, but the educated asses to whom the paraphrased lines from Hudibras fitly apply:

"Full many a 'man' returns from school
A Latin, Greek and Hebrew fool;
In 'sanitary knoweldge' still a block,
Tho' deeply skilled in Hic, Haec, Hoc."

We have reached that stage where the sanitarians and medical men of the country must realize that the existence of a contagious or infectious disease in any community is the sanitary remorse of that community, not only of desecration of the sacred altar of Hygeia, but of criminal neglect to educate the masses and to give them that public instruction in sanitary science which it is the highest duty of the state to afford them; for *"salus populi suprema est lex."* Therefore, I sincerely hope that the resolution which I introduced this morning will be unanimously passed by the Executive Committee. This resolution, in a general way, endorses the recommendations set forth in the paper which has just been read; commends the object and purposes of our State Sanitary Association; the advancement of sanitary science and popular education in the cause and prevention of contagious and infectious diseases in man and domestic animals.

DR. J. N. HURTY, of Indianapolis, Ind.—I trust the members of the Association understand that the report I have read is not mine, and that therefore it is not immodest to take part in this discussion. I shall take this opportunity of saying that in Indiana we have been using this method for four years of lecturing before the public whenever the opportunity could be secured. We have teachers' institutes that hold meetings in every county once a year. I believe we have ninety-two teachers' institutes altogether. Furthermore, we have ninety-two farmers' institutes, one in each county. We have had so many invitations in the last year from these different bodies to hear us upon the subject of sanitation that we have not been able to meet one-tenth of them. We have only two lecturers, and their time is well occupied in lecturing to the people. In addition to these institutes, we have other societies. Even the Knights of Pythias societies have called upon us twice for lectures on sanitation. So, it seems to me, the people are hungry for sanitary knowledge. They like to have the lectures illustrated, if possible, and I am proud to say, the Board of Health of Indiana has had made a large number of slides for the purpose of illustrating lectures upon hygiene. We furnish lanterns for these lectures. Of course, we have been severely criticised by the politicians on account of the expense, but nevertheless the work has gone on in spite of what these politicians have said. We understand that politicians can furnish sums of money for almost any job, but when it comes to furnishing money for charitable purposes, for advancing and educating the people, it is always a question with them as to whether the money should be expended. I believe this work should be done, and it is being done successfully. We have gotten the various teachers' institutes and farmers' institutes to pass resolutions approving this work, and we have sent these resolutions to the members of the legislature and to the Ways and Means Committee. The members of the legislature have become convinced that this work is really popular, so that many of them are now in favor of it. We expect good things from the next legislature. The members understand that the people are anxious for this knowledge on sanitation, and they want the people's votes.

Dr. Juan Guiteras, of Havana, Cuba.—I hope, in the subjects that are included in this system of teaching hygiene to the public, that it will not be forgotten, especially throughout the South, that it has recently been discovered that two of the most important diseases are introduced by the skin. It seems to me, in the plan that has been outlined in the paper just read that point has been overlooked—the importance of the skin as a means of introduction of infectious diseases. It should be remembered that malaria and yellow fever are introduced by the skin, and a study of the mosquito introducing these two diseases should be throughout the South a very important educational feature.

Dr. C. V. Chapin, of Providence, R. I., read a paper entitled "Dirt Diseases and the Public Health." (See page 296.)

DISCUSSION.

Dr. Quitman Kohnke, of New Orleans, La.—Mr. President: But a few years ago it would have required considerable courage for any man, claiming to be a sanitarian, to have voiced the substance of this paper. It requires less courage today, because we know more about the real dangers, and we can afford to say today what might not have been said perhaps ten years ago. It would still, in my opinion, be somewhat hazardous to voice such sentiments in an assembly, claiming no expert knowledge, and not properly prepared, as the members of this assembly are, to receive such ideas. Municipal sanitation is not the most important sanitation, but it marks an epoch, which includes the essentials. Street cleanliness is not as necessary as personal cleanliness. I believe that, while we are ready to receive a paper of this kind, and judge it upon its merits, it would as I have said before, be rather hazardous to say these things to people who would not thoroughly understand them. I believe that an organization of this kind should insist upon all kinds of cleanliness as being necessary. Cleanliness is a general drag-net that includes everything, the good, the indifferent, the necessary, the unnecessary, in the hope that that which is most necessary will be done along with the unnecessary. This is from the point of view of effectiveness.

The sanitarians of this city for years past had no question more important than the prevention of yellow fever, which was for many years believed to be a filth disease. We now know that it is not a filth disease, but it was necessary at the time, when the crusade in favor of municipal cleanliness, in favor of sewerage, drainage, and a pure water supply, was on, to preach the doctrine that yellow fever was a filth disease, and by adopting plans presented we would free the city from yellow fever. It is true that no medical man preached this doctrine, but we did not publicly contradict it. If the ideas enunciated by Dr. Chapin had been promulgated by those who were advocating sewerage drainage, and a pure water supply for this city, it is somewhat doubtful whether the mass of people, who voted for these improvements, would have come to the front and given this city what is promised us now.

Dr. Augustin Chacon, of Mexico City, read a paper on "Oculists and Opticians." (See page 311.)

Mr. Robert Spurr Weston, of Boston, Mass., read a paper entitled "The Water Supply of New Orleans and Its Improvement." (See page 278.)

DISCUSSION.

Dr. Charles A. Lindsley, of New Haven, Conn.—Mr. President: I would like to inquire of Mr. Weston what effect the coagulum has, if any?

Mr. Weston.—I do not think it has had any effect, so far as I have been able to discover. The coagulum in the English filter is used only to reduce the suspended matter to a point where the filter can handle it, not, as in the American filter, as an aid to the retention of the suspended matter by sand.

Dr. Quitman Kohnke, of New Orleans, La.—I desire to say for the information of Mr. Weston, as well as other members of the Association, that the report of the water purification experiments will doubtless be printed, and be procurable by those who are interested in such matters. The proper committee of the Sewerage Board will recommend its publication. The completion of the works destined to supply this city with an adequate amount of pure water will, in the opinion of most of those who are interested in such a subject, be of extreme importance.

There is one difficulty with regard to the Mississippi River to be met with, which was not included in Mr. Weston's paper, and that is the salt from the gulf. It may seem strange that a city so remote from the sea should have in the river, before its doors, as it were, the salt of the sea; the conditions being that the surface of the river is higher, always, than the surface of the gulf. It is explainable in this way: The bar at the mouth of the river permits the entrance of salt water, which, being heavier than the fresh water, sinks to the bottom. With a full tide, a fresh supply of this heavy water flows into the river, and crawls up, so to speak, on the bottom, until the waters eventually mingle, when it rises to the top and presents the phenomenon of salt water in a fresh water river at a higher surface than the level of the sea.

In Mr. Weston's closing remarks, I trust he will say a few words with regard to this, and suggest how this difficulty may be met, and whether or not such a contingency may be provided for in the near future. It is true, that this phenomenon occurs, perhaps, once only in twenty years; but when it does occur, it lasts for about a week or two, during which period it would be a hardship to our citizens were they compelled to drink salt water. Perhaps by the time the supply system is completed, even the salt in the water may be removed; by electrolysis, or in some other way, and it is to be hoped that this possible difficulty may be met successfully.

Dr. Gardner T. Swarts, of Providence, R. I.—I understood the reader of the paper to say that the area of an English filter would have to be 10 acres, while that of an American filter would be only 0.32 of an acre. I would like to ask him what is the depth of the river at the point of intake, and if the hard water referred to has anything to do with the accumulation of chlorine.

Mr. Weston (closing the discussion).—Regarding the depth of the river at New Orleans it may be said that it varies from about 90 to about 160 feet, the average being not far from 130 feet. The river, like all alluvial streams, flows over a succession of bars with holes between them, and while the depth of water between New Orleans and the jetties does not vary greatly from the limits given above, there is only a maximum of thirty feet of depth at the bar. This bar, of course, is formed by the precipitation of the water-borne suspended matter by the salt water of the quiescent gulf. Owing to the differences in specific gravity and perhaps also to the fact that at periods of minimum flow, the volume of water is not great enough to discharge the whole quantity of water, and at the same time fill the whole cross-section of the stream, the salt water of the gulf occasionally backs up along the bed of the stream from the gulf. This salt water, as Dr. Kohnke has said, probably fills the holes in the bed of the stream one after another, and passes up the bottom of the river as far as New Orleans, and probably above. This phenomenon probably occurs, as nearly as can be ascertained, at least once in twenty years.

There is little record of the amount of sea salt contained in the river water during these periods. Careful records have been kept at the laboratory of the Sewerage and Water Board since October, 1900, and as yet no amounts of salt have been detected above those which would be expected to be present in the river, even if it discharged into the gulf with a very much greater slope than has existed during the recent low water periods.

If memory serves correctly, Professor Metz detected as much as thirty-five grains of salt per gallon of the river water during one of these periods of minimum low water. While this amount of salt could be tasted, it is considerably less than that contained in the palatable table water which is now served on the dining cars of the Southern Railway under the name of Bowden lithia water.

From an engineering and economic standpoint, therefore, it seems injudicious to provide long periods of storage or to make any provision for the exclusion of this salt water, because even if it should be present every year and to a greater degree than has been recorded heretofore, the conditions would still be better than they are at some cities along the shores of the North Sea,—notably Rotterdam, at which city water has to be taken onto the filters during ebb tide, in order to prevent the accession of salt.

The hardness of the water is due to the salts of calcium and magnesium. These salts are largely carbonates. For a western river the Mississippi is only moderately hard.

DECEMBER 12—FOURTH DAY—MORNING SESSION.

The Association met at 10:30 A. M., and was called to order by the President.

THE PRESIDENT.—We will now listen to a report of the Executive Committee by the Secretary.

THE SECRETARY.—The Executive Committee has considered the various resolutions that were referred to it. Regarding the resolution offered by Mr. Leighton at a previous session as to exhibits of public health matters at the Louisiana Purchase Exposition, the Executive Committee recommends that a committee be appointed to look after that matter, and that the resolution be referred to that committee.

On motion, the recommendation of the Executive Committee was concurred in.

THE SECRETARY.—Two resolutions were submitted to the Executive Committee relative to the death of Dr. Walter Reed, and the Executive Committee has amended and adopted one, recommending to the Association that it be substituted for and take the place of the two resolutions.

This resolution reads as follows:

In the recent death of Major Walter Reed every branch of medicine has cause for sorrow. Preventive medicine, and especially laboratory workers in hygienic lines, must deplore their loss most keenly. It has been well said that for one hundred years no greater work than his has been done. His contributions to the study of yellow fever are worthy to rank with the work of Jenner on smallpox, and Koch's work on tuberculosis.

Major Reed was a Virginian, and represented in every respect the well-known ideal of a Virginian gentleman. Those who knew him personally were impressed invariably with his kindness, courtesy and modesty, while no greater testimony to the courage of any man than the record of his work in Cuba could be given.

Graduating from the University of Virginia, and from Bellevue Hospital Medical College, he became assistant surgeon in the army, and finally major in the medical department. In 1893 he was appointed curator of the Army Medical Museum, which position he occupied at the time of his death.

During the Spanish-American war he was connected with the typhoid fever investigation, and in 1900 began his remarkable work on yellow fever. Admirably planned, the results obtained were conclusive, valuable alike in theoretical deduction and in practical application; therefore, be it

Resolved, That a copy of this resolution with its preamble be forwarded to the widow of the late Major Walter Reed, U. S. A.

On motion, the preamble and resolution were adopted.

The Secretary.—The Executive Committee recommends for adoption the resolution relative to illuminating and fuel gas, and the dangers therefrom, as amended by the Executive Committee. This resolution, as amended, reads:

Resolved, That the American Public Health Association hereby recommends that the construction of systems of piping for the distribution of illuminating and fuel gas in buildings shall be placed under competent official supervision.

On motion, the recommendation of the Executive Committee was concurred in.

The Secretary.—The Executive Committee recommends that the resolution handed in by Dr. Williams relative to tuberculosis be referred to a committee to be created on the Prevention of Tuberculosis.

The President.—The Chair will appoint this committee later on, and this resolution will be referred to that committee.

The Secretary.—The Executive Committee recommends the adoption of the resolution in regard to the late Dr. Kedzie.

On motion, the recommendation was concurred in.

The Secretary.—In regard to the resolution presented by Dr. Williams, in connection with the report on Car Sanitation, the Executive Committee recommends that it be referred to that committee.

The President.—There being no objection, it is so referred.

The Secretary.—The Executive Committee recommends the adoption of the resolution offered by Dr. McGehee.

On motion, the recommendation was concurred in.

The Secretary.—The Executive Committee would recommend that the resolution presented by Dr. Mayer relative to public instruction in reference to the spread of tuberculosis, typhoid fever, and other infectious diseases, be referred to the Committee on Teaching of Hygiene, etc.

The President.—And it is so referred.

The Secretary presented a communication from Professor Sedgwick, which was referred to the Committee on the Teaching of Hygiene and Granting of Diploma of Doctor of Public Health.

The President.—We will now listen to the report of the Auditing Committee, which will be read by the Secretary.

REPORT OF AUDITING COMMITTEE.

New Orleans, La., Dec. 10, 1902.

The Auditing Committee has examined the books of the Treasurer, vouchers and reports, and find them correct.

Respectfully submitted,
J. N. McCormack,
Charles S. Caverly,
Committee.

On motion, the report of the Auditing Committee was adopted.

The Secretary read the report of the Advisory Council, as follows:

REPORT OF THE ADVISORY COUNCIL.

The Advisory Council recommends the following officers for the ensuing year:

President—Dr. Walter Wyman, Washington, D. C.
First Vice-President—Dr. C. P. Wilkinson, New Orleans, La.
Second Vice-President—Dr. John L. Leal, Paterson, N. J.
Treasurer—Dr. Frank W. Wright, New Haven, Conn.
Secretary—Dr. C. O. Probst, Columbus, O.
For Three Members of the Executive Committee—Dr. John S. Fulton, Baltimore, Md.; Dr. Jóse Ramirez, Mexico, and Dr. John A. Amyot, Toronto, Can.

Washington, D. C., is recommended as the place for holding the next meeting.

On motion of Dr. Mayer, the rules were suspended, and a ballot cast for each of the officers nominated, all of whom were declared unanimously elected.

THE PRESIDENT.—The question now is with reference to the place of meeting, as recommended by the Advisory Council. What action will you take in regard to that?

DR. FELIX FORMENTO.—I move that the recommendation of the Advisory Council in regard to the place of meeting be adopted. Seconded and carried.

THE PRESIDENT.—The Chair will appoint Drs. Durgin and Lachapelle to escort the President-elect to the platform.

The retiring President said:

Gentlemen of the Association: As your retiring President, I have great pleasure in presenting to you Dr. Walter Wyman, Surgeon-General of Public Health and Marine Hospital Service, who has been elected as your President for the ensuing year. (Applause.)

Dr. Wyman, in accepting the Presidency, said:

Mr. President, and Gentlemen of the Association: I can scarcely find words to express my appreciation and gratification at the great honor which you have conferred upon me. Surely, it is an honor that anyone might be proud of to have his name enrolled upon the list of those who have been Presidents of the American Public Health Association. But it is more than a personal gratification to me. It means much more. I am of the firm opinion that through recent legislation, through the action of several bodies of physicians, and particularly this Public Health Association, that there may be brought about such a unification of the efforts of physicians and sanitarians that in due course of time we shall achieve results which now seem very difficult of attainment. As you know, there are several associations that are bent upon doing work similar in character to the objects and aims of this Association. The American Medical Association has now become a potent body in these matters. It has a legislative body limited to one hundred members, which gives it far greater weight and power in enforcing matters relating to the medical profession. There is a national convention of state boards of health which meets annually, which is a powerful body and is working along the same lines as this Association. Then, there is the conference of state boards of health, which is called for by the act of July 2, 1902, subject to the call of the Surgeon-General of Public Health and Marine Hospital Service. Finally, there is, before we come to this Association, an international body, organized in accordance with the findings of the congress of American states, held in Mexico last winter. This is a delegate body, whose delegates represent the governments who send them to join in a conference regarding sanitary matters affecting the several republics, and then there is this great American Public Health Association which for years has been the fountain source of all the public health work, the stimulating force, at least, of all the public health work that has been done in this country. I have received my inspiration in all health matters from attending the meetings of this Association years ago, and in reading its published reports. I look forward, therefore, with pleasure to the coming year in taking an active part, which I shall do, as President, and endeavor to unify the operations of these several bodies, so far as may be in my power, so that we may all work together upon planes that are harmonious and systematic. We must feel, first, that we are right in everything, and perhaps in the way of public health legislation or of public health work, the old David Crockett maxim, "Be sure you are right, then go ahead," is one of the best that can be adopted by those who are interested in public health work. And if you will notice in that motto, there are more words in the first part, "Be sure you are right," than there are in the latter, "then go ahead." The greatest difficulty is in being sure you are right, in determining absolutely that you are right with your measures and ways, which will stand the test of time and criticism. After that, the going ahead is comparatively easy.

Gentlemen, I again thank you for the distinguished honor you have conferred upon me. (Applause.)

Dr. J. N. McCormack, of Kentucky, offered the following resolution:

Resolved, That the cordial thanks of this Association be and are hereby tendered to the Committee of Arrangements, the medical profession, and the ladies of New Orleans for the ample and most hospitable provision made for the comfort and entertainment of the members of this body and its guests, which has made this one of the most pleasant and successful meetings in its history; to the newspapers of the city for the full and accurate reports made of its proceedings, and to the various transportation companies for reduced rates, and to the retiring President for the able and impartial manner in which he has presided over our deliberations.

On motion, the resolution was unanimously adopted:

DR. FAGAN, of British Columbia.—There is one matter that I bring before the Association with hesitation. In reading the different medical journals I find the conventions of the boards of health of the United States have taken very active measures with regard to a question that affects me, and the district I represent, more actively than probably any other part of this country. I refer to the existence of bubonic plague in San Francisco. It seems to me, not only a pity, but a great mistake, if this Association should adjourn without mentioning what is being done there. We are all familiar with the history of the unfortunate condition that has existed there since March, 1900.

THE PRESIDENT.—A committee on plague was appointed yesterday, to take this matter into consideration.

DR. FAGAN.—I understand that, and I believe I have been asked to serve on that committee, but we must remember that it takes a year for this committee to make its report, and we do not know what is going to happen in the meantime. Plague has existed in San Francisco since March, 1900, in the bubonic form. While I was on my way from San Francisco to this convention, I was told on good authority that pneumonic plague was now existing there, and, what is worse still, the rats are now affected. It would be a great mistake, in my opinion, if this Association does not take some action or recommend some measures to be adopted as an authoritative body. Certainly, the conditions, from what I can learn, are deplorable, and in British Columbia we feel that we are greatly endangered, because we all know that in San Francisco they are doing practically nothing. While the disease existed in bubonic form, there was less danger, but now the rates are becoming affected, and are developing pneumonic conditions, the situation is serious. I feel a certain amount of diffidence in bringing this matter before the Association, inasmuch as I do not belong to the United States. I do not propose to offer any resolution, nor to suggest any measures that might be taken.

DR. WALTER WYMAN.—The gentlemen may not know that, in accordance with a law passed last July, the Surgeon-General of Public

Health and Marine Hospital Service is required under that law to call a conference whenever it is requested by five state boards of health. I have received requests from state boards of health, urging that such a conference be called during the Christmas holidays, but the exact time for calling it has been deferred until it is determined when will be the best time. I think this matter to which the gentleman (Dr. Fagan) has referred can be safely left to that conference.

DR. FAGAN.—I feel absolutely satisfied after what Dr. Wyman has said, and I trust such a conference will take place at an early date.

THE SECRETARY.—I am requested to announce the following officers of the Section of Bacteriology and Chemistry for the coming year: *Chairman,* Dr. H. L. Russell, Madison, Wis.; *Vice-Chairman,* Dr. V. A. Moore, Ithaca, N. Y.; *Secretary,* Mr. G. C. Whipple, Brooklyn, N. Y.; *Recorder,* Dr. H. D. Pease, Albany, N. Y.; *Councillors,* Dr. W. H. Park, New York City; Dr. M. J. Rosenau, Washington, D. C.; Mr. E. G. Horton, Columbus, O.; Dr. Angel Gaviño, Mexico City, Mexico, and Dr. F. F. Wesbrook, Minneapolis, Minn.

A paper by Dr. Eduardo Licéaga, of Mexico City, entitled "Anti-Rabic Institute of the City of Mexico," was read by title, and referred to the Publication Committee.

A similar disposition was made of papers by Drs. Juan Guiteras, of Havana, Cuba, and H. C. H. Herold, of Newark, N. J.

The President announced as Chairman of the Committee of Arrangements for the next meeting Dr. W. C. Woodward, of Washington, D. C., with power to select associates. The time of the meeting will be left with the President, Secretary and local committee, and in due season a preliminary circular will announce the date of the meeting.

Before adjourning, retiring President Holton said: I desire to express my gratification of the kind support you have given me during this meeting. I do not remember any meeting of the Association that has gone along any more pleasantly and smoothly, and it is not because of the presiding officer, because I feel that without your generous support and your kindness I should not have done so well. I trust we may have many meetings in the future that will be as pleasant as this one has been, and I have no doubt the meeting next year will be a greater success than this has been. (Applause.)

On motion of Dr. McCormack, the Association then adjourned, to meet in Washington, D.C., 1903.

PROBLEMS IN THE LABORATORY STUDY OF DIPHTHERIA.[1]

By F. F. WESBROOK, M. D., Minneapolis, Minn.

Director Minnesota State Board of Health Bacteriological Laboratory, Professor Pathology and Bacteriology, University of Minnesota.

In contemplating the position attained and the work accomplished by the laboratory section of the American Public Health Association in its brief history and remembering the high standing of my predecessors in office, I realize how undeserved is the honor which you have done me. I could not have accepted this position as your chairman did I not feel that your action was more in the nature of a tribute to the progressive board of which I am one of the representatives, than a personal matter. For the Minnesota State Board of Health and on my own behalf, I desire to assure you how very much that honor has been and is appreciated.

In selecting a subject upon which to address you I have been influenced by divers considerations of which not the least important is my belief that to few is given the ability to interest and profit others by a review of recent advances in science, or other similar general topics, and that your present chairman is not one of the few.

It has been one of the foremost aims of this section—was, in fact, a tradition handed down through the Association by our late esteemed chairman, Dr. Wyatt Johnston—to secure uniformity of methods and standards whereby results in the various public health laboratories could be readily compared. Hitherto this process of standardization has been largely limited to the study of laboratory problems in the investigation of water. Recently, however, attempt has been made outside this section to investigate by standard methods and in a systematic way certain phases of another subject which forms a large part of the routine work of many public health laboratories. I refer to the exhaustive investigation so successfully instituted and carried out by the committee of the Massachusetts Association of Boards of Health[2] especially appointed for the study of diphtheria bacilli in well people.

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[1] Chairman's address, Laboratory Section, American Public Health Association, New Orleans, La., December 8, 1902.

[2] Journal of the Massachusetts Association of Boards of Health, July, 1902.

Informal discussion of the results obtained in various localities on this continent by the different collaborators—most of whom are members of this section—indicated that there was need for the formulation and adoption of standard methods in the laboratory diagnosis of diphtheria. This subject was therefore suggested as a fitting one for the present meeting, and although I felt somewhat diffident about bringing it before your notice at the present time, I was induced to do so for two reasons:

First—The diagnosis of diphtheria in relation to restrictive measures has constituted and still constitutes one of the most important parts of the routine work of many public health laboratories, and the necessity for immediate executive action throws the main, if not the whole responsibility upon the results of the laboratory investigation and the interpretation thereof.

Secondly—The presence of *Bacillus diphtheriae* in the throats and noses of the apparently healthy has led some to question the value of laboratory work in this disease, and for fear that we may be led to adopt erroneous conclusions and act too hastily, it appeared desirable to carefully consider the data at present available, and if possible from it to define exactly the status of the laboratory in relation to the diagnosis and control of diphtheria.

If the facts at hand appear to be insufficient for a final statement, we may very properly consider what has yet to be determined in the way of data, and formulate suggestions as to their acquisition.

BACILLUS DIPHTHERIAE IN WELL PERSONS.

The occurrence of *Bacillus diphtheriae* in well persons has perplexed health authorities for many years, and many laboratories have endeavored to gain an idea of the extent of such infection.

The Minnesota State Board of Health, like others, had been studying the matter for a long time, and contemplated seeking for collaboration with others interested as soon as some basis for tabulation and comparison of results could be hit upon. Before the plans were completed, however, the Association of Boards of Health of Massachusetts inaugurated and carried out the work with a thoroughness which was only possible because of its admirable organization and by reason of the zeal and energy of its officers.

An analysis[1] of the results of this committee's work is impossible within the limits of the present communication, but a few points may be brought out by a study of their tables.

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[1] Journal of the Massachusetts Association of Boards of Health, July, 1902, tables IX and X, "Report on diphtheria bacilli in well persons."

1. A comparison of the relative frequency of occurrence in the nose and throat shows that in general the most commonly recognized types of diphtheria bacilli occurred about as frequently in the throats as in the noses of the healthy people examined, whilst the solid and barred types were with few exceptions much more abundant in the nose. In other words, the barred and solid types (Xerosis and pseudo-diphtheria bacilli) are essentially nose bacteria so far as healthy individuals are concerned.

2. Great differences were observed in the numbers of well persons infected with the various types by the various collaborators. These differences are very striking and cannot be attributed alone to differences in technique or classification, but are most probably due to local conditions. It would appear that at least some of the differences in results were due to the fact that in some sections of the country the cultures examined were taken from certain classes of people, whilst in other sections, other and widely different classes were examined.

Minnesota shows a far higher percentage of infection than do most of the other localities. This is particularly noticeable in those large granular types which are so characteristic of clinical diphtheria. I refer to types D., C. and A. In New York alone was the percentage of throat infection with these particular types greater than in Minnesota, and I understand that diphtheria was prevailing at the time in the particular district in New York where the cultures were taken.

A comparison of the findings in the different localities examined in Minnesota before, during and since the Massachusetts investigation serve to throw light upon the high percentage reported from Minnesota during this investigation.

In Minnesota we find that in institutions especially where children are housed the percentage of people infected with the most typical forms of diphtheria bacilli is relatively high, varying from 4 to 25 per cent., according to whether there has been recent exposure to clinical diphtheria or not.

In day-school children we found in examining recently over 230 children during an almost universal epidemic of sore throat due to streptococcus, that the infection with these large well recognized granular types of diphtheria bacilli varied between 1 and 2 per cent., even though there had been and were at the time prevailing in this small country town some cases of clinical diphtheria which were verified by laboratory findings. Some exposure to clinical diphtheria probably increased this total infection, low as it was.

Minnesota's contribution to the Massachusetts investigation included reports upon the examination of cultures from five institutions in which over 800 children were housed, and cultures from over 300 public day school children in a small country town in which diphtheria had been epidemic during the preceding winter and summer, and was at that time prevailing and constituted the reason for the investigation. Incidentally it may be stated that exclusion of *all* children who showed the large granular types from attendance at school was followed by a prompt disappearance of the disease, although it was at the beginning of the fall term, and the opening of school had been greatly dreaded.

Since in Minnesota, where variations due to differences in technique, personality of observer, climatic and other conditions can be eliminated, such great differences in results were observed in different investigations, it seems fair to assume that a close analysis of the Massachusetts results would reveal a much lower percentage of infection with the well recognized types of diphtheria bacilli in ordinary people. *We are perhaps also warranted in believing that the matter of the infection of the well inmates of institutions constitutes a problem to be considered quite apart.*

3. A comparison of the types and percentage infection found in well people with those found in clinical diphtheria is of interest. So far as I am aware no statistics concerning the distribution of the various types of *Bacillus diphtheriae* which occur in clinical diphtheria are available, with the exception of those compiled from the routine examinations of throat cultures in Minnesota for 1899-1900. It therefore becomes necessary, for the purposes of accurate comparison, to utilize only the statistics of Minnesota which relate to the occurrence of diphtheria bacilli in well persons, and I am forced, therefore, to exclude the results of the other examinations made in collaboration with Massachusetts.

The Minnesota examinations afford, however, perhaps sufficient data to warrant the drawing of a few tentative conclusions, although it is possible only to compare the findings in the throat cultures, since in routine examinations of clinical diphtheria nose cultures have not been studied.

Such comparison shows amongst other things:

(*a*) In the throats of clinical cases of diphtheria there is a greater variety of types met with than in well people.

(*b*) As might be expected, the throat infection with the generally accepted large granular types D., C. and A. is 30 to 40 times as great in clinical cases as in the children in institutions, and 60 to 100 times as great as in the throats of the day school children.

(*c*) The solid types E^2 and D^2, frequently known as Hoffman's bacillus, which are so common in the noses of the well children, are not very numerous in the throats of the institution children (13-14 per cent.), and amount to less than 1 per cent. in the day-school children, whilst in clinical cases they are very abundant (39-55 per cent.). This would seem to suggest that during clinical diphtheria these types—essentially nose types—have a tendency to extend from the nose and take up their residence in the throat, or that they are in the process of evolution into granular types. On the other hand, in those day-school children suffering with streptococcus sore throat they seemed to have disappeared altogether from the throat and not to have increased threefold, as is the case in clinical diphtheria.

From the data at present available it would appear that:

The percentage of infection of the noses and throats of well people with diphtheria bacilli varies within rather wide limits.

A critical analysis shows that the types of diphtheria bacilli characteristic of clinical diphtheria are not so abundant in well people as was expected.

Where well people have been exposed to clinical diphtheria the infection is undoubtedly much higher, so far at least as the granular or generally recognized types are concerned.

Where conditions favor the exchange of the bacterial contents of nose and throat, the infection in well people is higher than where this is not the case. For this reason the percentage of infection in well children is higher than in adults under similar conditions. On this account also, in the inhabitants of institutions where children live continuously, the infection is found to be greater than in day schools. Probably this factor should also be taken into account in considering the matter of the increase in clinical diphtheria coincident with the opening of schools in the autumn, when large masses of children are brought together after the comparative segregation of the summer.

In clinical diphtheria, so far at least as the Minnesota results of 1899-1900 can be taken as an index, the granular types of bacilli are found in over 90 per cent. of the examinations.

On this basis, therefore, the barred and solid types of bacilli, when unmixed with the granular or well recognized types may perhaps be looked upon in the meantime as merely suspicious, and other cultures and more particulars asked for. Should examination of subsequent cultures fail to show the granular types of bacilli a negative report may be given, although for purposes of study the profession at large should be taken into the confidence of the laboratory workers and its assistance asked in the collection of data.

So far as the relationship of the members of the barred and solid groups of types of bacilli to the granular types is concerned, much has still to be learned. Many observers believe in the possibility of the change of one type into others. This need not be entered into here as the recently published works of Williams,[1] Gorham,[2] Ohlmacher,[3] Le Sieur[4] and the Minnesota State Board of Health Reports[5] all bear upon the question.

The facts presented here to-day show that the barred and solid types, although relatively abundant in the noses of healthy individuals, are not nearly so prevalent in the throats of well people as in the throats of clinical cases of diphtheria. This would seem to require explanation. In the laboratory of the Minnesota State Board of Health work is going on along two lines which it is hoped will throw light upon this question:

(*A*) The strictly laboratory or biological line, in which endeavors are being made by Dr. O. McDaniel to find a means of changing one type into another. Amongst the methods in use is that of growing the solid or barred types of bacilli in fluid media contained in dialyzing sacs, which are suspended in broth in which the virulent granular types have been planted. The celloidin sac in animals is also being used in various ways. This work has been unfortunately interrupted, though we hope to resume it shortly.

(*B*) Exact daily observations by uniform methods on daily cultures from the throats and noses of clinical cases in hospital. Dr. L. B. Wilson has been engaged in this work for several months, and has devised a method of graphically recording the types of bacilli met with, and of showing the variation of the types when remedial agents such as diphtheria antitoxin are employed. Public health laboratories are so helpless in the matter of securing full and reliable collateral clinical and therapeutic data that a special study of hospital cases seemed necessary.

There still remains, however, a good deal of work to do which may afford valuable information. To secure the best results collaboration of laboratories which are situated in widely distributed districts is necessary. Relatively little except in a general way is known concerning the morphological types of bacilli which are met with in clinical diphtheria. In Minnesota we have tabulated the types of bacilli met with in the routine examination of 608 clinical cases in 1899-1900, and have nearly completed those for 1901-1902. These results can only give us an idea of the distribution of types in Minnesota, and it would appear to me to be desirable to ascertain what types are found in other districts. The Massachusetts investigation has afforded much valuable data concerning the types found in well persons, but we should endeavor to obtain, if possible, a more exact idea of the findings upon which we base our opinions every day.

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[1] Williams: "Persistence of Varieties of the Bacillus Diphtheriæ and of Diphtheria-like bacilli." *The Journal of Medical Research*, June, 1902, p. 83, et seq.

[2] Gorham: "Morphological Varieties of Bacillus Diphtheriæ." *The Journal of Medical Research*, July, 1901, p. 201.

[3] Ohlmacher: "Observations upon the Morphologic Variations of Certain Pathogenic Bacteria." *The Journal of Medical Research*, January, 1902, p. 128.

[4] Le Sieur: "Les bacilles dits "Pseudo-diphteriques." pp. 951 and 1000, *Journal de Physiologie et de Pathologie-General.*

[5] Minnesota State Board of Health Report, 1899-1900, p. 581, *et seq.*

It would appear to me that by this means we may be placed in a position to standardize our methods, whilst at the same time much of value may be learned concerning the distribution of the various types.

If this question appeals to the section, I would suggest that work in committee be undertaken for the purpose of collecting information and of later making recommendations as to the best methods, even to the preparation of media, employment of stains and other technical matters, whilst it will be of the utmost value if some morphological standard for diagnosis may be set.

This latter is important, since from all appearances the laboratory diagnosis of diphtheria must continue to depend on morphology, as insufficient time for cultural and biological study is available.

In the meantime, whilst studying these problems which frequently at first sight seem to give us conflicting results, we should ever keep before ourselves and others our true position, namely, that of searchers after truth, and the application of these truths to the control of infectious disease.

I have refrained from any discussion of what should be done in the way of restrictive measures for well people in whom diphtheria bacilli are found, because it is strictly an executive matter, and because also of the hope that in the process of obtaining further and more exact knowledge of the types of diphtheria bacilli in clinical cases we may be afforded information of use in solving this question.

REPORT OF THE COMMITTEE ON STANDARD METHODS OF WATER ANALYSIS. THIRD REPORT OF PROGRESS.

The Committee on Standard Methods of Water Analysis has given its attention during the past year chiefly to bacteriological work, and in this Third Report of Progress only two topics will be taken up.

1. A comparative study of gelatins used in quantitative bacteriological investigations; and
2. Comparative studies of unknown species.

COMPARISONS OF GELATINS.

The following description of experiments conducted upon this subject was published in the form of "Advance Notes," a copy of which was mailed to each member of the Section. A copy of these "Advance Notes" is appended and forms a part of this report.

The results obtained in this comparative study led to a further investigation, on the part of the secretary, of the physical properties of gelatin with reference to its use in culture media. These studies were published in the Technology Quarterly, Vol. 15, No. 2, June, 1902. They included a study of viscosity, spissitude and melting temperature, and the effect upon these physical properties of different methods of sterilization. It was found that increased periods of heating lowered the melting point and decreased the spissitude, and it was found that the value of the gelatin decreased with spissitude and melting point. Comparisons were made showing the relative effect of sterilizing in the Arnold sterilizer and the autoclav, and an attempt was made to discover how uniform results could be obtained by both methods. It was found that different gelatins varied in their physical and chemical properties, and that the bacteriological results obtained by using different gelatins were influenced thereby. This was true not only in quantitative but in qualitative tests. In fact when studies were made of some species of bacteria which liquefied the gelatin the appearance of the stab-cultures varied through a wide range, some gelatins giving a growth which was not only more vigorous, but entirely different in character from others. Illustrations of these growths are given in the paper mentioned. It is very apparent that our knowledge of gelatin is far from perfect. Certain it is that the character of this substance may be a very variable quality. It has been learned that even the gelatin of a particular brand is not constant in quality, and even gelatin manufacturers admit that it is almost impossible to produce two batches of gelatin which are exactly alike. In order to obtain a gelatin of uniform quality, it is not sufficient to specify some particular brand. The only way, apparently, in which it can be obtained is to get some manufacturer to prepare a special mixture of gelatin for bacteriological work, and to have him make this in such quantities that it will last the members of the profession for a considerable period of time. This, we understand, certain manufacturers are willing to do, and your committee recommends that steps be taken towards securing such a standard gelatin for use in bacteriological work. It is believed that if such a gelatin were placed on the market and received the endorsement of this body, it would become generally used. It is proposed to have gelatins from different manufacturers tested by the committee, and that one selected which gives the most favorable results. The manufacturers claim that if sufficient time be given, say a period of several months, it will be possible to practically duplicate the gelatin selected when its supply shall have become exhausted.

COMPARATIVE STUDIES OF UNKNOWN SPECIES OF BACTERIA.

The qualitative examination of unknown species of bacteria, which was begun during the year 1900-1901, has been continued during the present year. Two species, referred to as "C" and "D," were sent to various laboratories for comparative tests. Eleven bacteriologists worked these out, and submitted their results to the committee. These results were tabulated, and a blue print copy mailed to each one who participated in the work. In studying species "A" and "B," the bacteriologists were directed to follow their own particular methods, but in the case of species "C" and "D," the committee recommended that the directions given in the 1897 report of the bacteriological committee be strictly followed. In comparing the results of cultures "C" and "D" with those of cultures "A" and "B," the advantage of adhering to standard methods was clearly shown. To some extent it would appear that species "A" and "B" were somewhat more erratic in their bio-chemical relations than species "C" and "D," but yet there is ample margin to indicate that the adherence to standard methods has a decided advantage. For the most part the results of the qualitative investigations of species "C" and "D" show a substantial agreement, but in several instances there were marked discrepancies in the results obtained. For example: In the indol test four observers stated that indol was produced by species "C," while six observers stated that it was not; three observers stated that species "D" produced indol, while eight stated that it did not. So also in the case of the production of acid in milk: Eight observers stated that acid was produced by species "D," while three observers stated that it was not produced.

While the general result of these tests may be considered as being fairly satisfactory, yet it is evident that the methods proposed by the bacteriological committee of 1897 do not entirely fulfill modern requirements.

REVISION OF THE 1897 REPORT OF THE BACTERIOLOGICAL COMMITTEE.

At the last meeting of the Section of Bacteriology and Chemistry held at Buffalo in 1901, the report of the bacteriological committee of 1897 was referred to the existing Committee on Standard Methods of Water Analysis, for revision. This matter has been taken into consideration by your committee, but no material progress has been made during the past year in the practical work of revision. Your committee expects to devote its time during the coming year to the preparation of a dual report, to include this revision of the 1897 report and its own work on standard methods of water analysis, and in order that the work of revision may be simplified and that it may include the best methods now known to our members, we are planning to assign various parts of the work to sub-committees for preliminary report, and in this work we cordially invite each member to furnish whatever information he possesses in reference to improvements in any of the tests outlined in the original report. Due credit will be given for any such suggestions. During the past five years, since the publication of the original report, several new methods of study have been introduced and new methods of classification have been suggested. It is proposed to incorporate these new methods in the final report as far as they have been tested, and proven to be of value.

THE MEASUREMENT OF THE TURBIDITY AND COLOR OF WATER.

Your committee desires to call the attention of the members to a modification of the present methods in measuring the turbidity and color of water, adapted to field work. These methods are described in a circular issued by the United States Geological Survey, this circular being No. 8, published by the Division of Hydrography. The method of measuring turbidity is a modification of the old wire method, which has been improved by studies made by Messrs. Hazen and Whipple, in order to make the readings obtained by the wire method correspond with those obtained by the use of silica standard. The method of measuring color includes the use of glass disks, made to correspond with the present platinum standard, and used according to illustrations which are here exhibited. Inasmuch as this Section has recommended the adoption of the silica standard for turbidity and the platinum standard for color measurements, it would seem appropriate to endorse these new methods.

Respectfully submitted,
GEORGE W. FULLER,
GEORGE C. WHIPPLE,
HARRY W. CLARK,
ADOLPH GEHRMANN,
E. O. JORDAN,
H. L. RUSSELL.
Signed for the Committee,
G. C. WHIPPLE,
Secretary of Committee.

COMMITTEE ON STANDARD METHODS OF WATER ANALYSIS. ADVANCE NOTES FROM THE THIRD REPORT OF PROGRESS.

COMPARISON OF CULTURE GELATIN.

In its Second Report of Progress, presented to the Section at the Buffalo meeting, the Committee on Standard Methods of Water Analysis suggested a mode of procedure for the preparation of nutrient gelatin and agar for quantitative bacteriological work. In order to test the value of these directions in securing a product which would give uniform results in practice the committee issued a circular letter asking that samples of gelatin be submitted for comparison. In response to this invitation eighteen samples of gelatin were received. These were tested as to their reaction, melting point, specific gravity and chlorine and their relative value for quantitative bacteriological work. The tests were made by the secretary, with the coöperation of Mr. R. B. F. Randolph, Assistant Director of the Hoagland Laboratory, Brooklyn, N. Y. The results were tabulated and blue print copies mailed to those who submitted samples. This report, somewhat condensed, was as follows:

SAMPLES.

Samples Nos. 1, 2 and 3 were received in test-tubes; the others, either in flasks or bottles. Sample No. 6 melted in transit, and a part of the gelatin escaped through the cotton plug. No sample was found to be contaminated.

The samples were first melted at about 40 degrees C., and portions removed for the determination of the reaction, after which other portions were tubed, using 5 c. c. in each tube. The tubed gelatin and what remained in the original package was then sterilized for 10 minutes at 110 degrees C. and cooled in ice water. On the following day a sample of Brooklyn tap matter was plated with each gelatin according to the method recommended by the committee; the melting point was determined for each gelatin, and determinations of chlorine and specific gravity were made. What was left of each gelatin was then heated for several hours and determination of melting point and reaction made at intervals.

The general results of the comparative tests are given in Table No. I.

TABLE No. I.

Sample Number.	Date of Preparation, Nov., 1901.	Sterilization.		Reported Reaction.	Color, L—Light; D—Dark; V—Very.	Specific Gravity.	Chlorine (Per cent.)	Melting Point (Cent.)	Observed Reaction.	Number of Bacteria Per C. C.	Per cent. of Plates which were Partially Liquefied.	Per cent. of Plates which were Wholly Liquefied.
		Temperature, (Cent.)	Time, (Minutes.)									
2	8	100	3x30	1.00	L.	1.0324	0.196	33.0	1.63	125	0	0
1	4	115	10	1.00	L.	1.0334	0.248	31.4	1.05	124	0	0
11	6	110	15	0.80	D.	1.0265	0.101	27.1	1.17	112	0	0
7	10	113	20	1.00	L.	1.0142	0.105	24.8	1.32	110	60	0
8	12	113	20	1.00	L.	1.0208	0.061	27.1	1.12	108	0	0
3	8	110	15	1.00	L.	1.0295	0.152	31.4	1.05	101	0	0
5	Oct. 31	100	3x30	1.00	D.	1.0250	0.134	21.5	1.53	98	20	0
12	6	100	3x30	*	L.	1.0346	0.205	26.2	1.88	87	0	0
16	6	100	4x30	1.00	V. L.	1.0300	0.140	24.8	0.56	81	0	0
4	7	100	3x—	1.00	D.	1.0298	0.318	23.8	0.95	80	—	40
14	4	100	4x15	0.95	V. L.	1.0310	0.235	26.2	1.98	78	0	0
17	5	100	3x30	1.45	L.	1.0344	0.201	23.8	1.52	72	60	0
10	11	110	2x15	1.10	D.	1.0308	0.174	24.8	1.13	55	—	40
13	—	—	—	—	V. L.	1.0289	0.097	23.8	1.32	44	40	60
9	7	110	15	1.20	V. D.	1.0353	0.192	23.8	-1.70†	29	100	0
6	7	100	3x20	0.78	D.	—	0.189	24.8	0.52	Liq.	—	100
15	—	—	—	1.00	D.	1.0247	0.144	23.0	1.93	Liq.	—	100
18	7	100	3x15	1.10	L.	1.0362	0.194	22.5	1.35	Liq.	—	100

* Reported as "Too Acid." † Probably Abnormal.

REACTION.

The reaction was determined as follows: Five c. c. of the sample melted at about 40 degrees C., was put in a porcelain evaporating dish with 45 c. c. of distilled water previously boiled to expel the dissolved carbonic acid. After boiling for about two minutes the titration was made with N-20 NaOH, using 1 c. c. of phenolphthalein solution as the indicator. The titration was made while boiling, or exactly at the boiling point, as it was found that if allowed to cool for even half a minute an exact end point was not obtained. The first indication of pink was considered as the end point. The results were made independently by two observers, and in some cases they were checked by a third observer. Whether or not this technique is the best that can be adopted, it served well to show the relative reactions of the different gelatins.

The results of the determinations are given in Table No. II, from which it will be seen that the reaction varies from 1.98 to 0.51. One gelatin had a reaction of —1.42, but this was evidently abnormal for some reason. Some of the gelatins were reported as either above or below 1.00 per cent. In order to compare the reaction on a basis of 1.00 per cent., therefore, the results with these gelatins were reduced to this basis by dividing the observed reaction by the reported reaction. These figures are given in the last column. Omitting Sample No. 9, the reactions were found to vary from 0.56 to 2 :09, the average being 1 :28.

These differences may be accounted for in part by changes in the gelatin during sterilization, but it seems probable that the greatest differences are due to different methods of titration, particularly as to the end-point, or to lack of exact manipulation by the preparers.

MELTING POINT.

The melting point was determined approximately by immersing one tube of each gelatin in a large water bath, where the temperature was kept constant at different points for periods of five minutes, the temperature of the bath being observed constantly. It was found that different gelatins behaved differently on warming. Some became soft at a comparatively low temperature and remained in a semi-fluid condition over a range of several degrees, while others changed from solid to liquid quite rapidly. The melting point was considered as that temperature at which the gelatin would flow down the tube when inclined at an angle of about 30 degrees below the horizontal.

TABLE No. II.

Sample Number.	Reported Reaction.	Observed Reaction.			Calculated End-point for Reaction of +1.00.
		By R. B. F-R.	By G. C. W.	Ave.	
1	1.00	1.03	1.07	1.05	1.05
2	1.00	1.64	1.62	1.63	1.63
3	1.00	1.02	1.07	1.05	1.05
4	1.00	0.97	0.90	0.94	0.94
5	1.00	1.53	1.53	1.53	1.53
6	0.78	0.47	0.54	0.51	0.67
7	1.00	1.37	1.27	1.32	1.32
8	1.00	1.12	1.12	1.12	1.12
9	1.20	—1.74†	—1.66†	—1.70†	—1.42†
10	1.10	1.09	1.17	1.13	1.03
11	0.80	1.17	1.17	1.17	1.44
12	1.00	1.83	1.93	1.88	1.88
13	—*	1.37	1.27	1.32	1.32
14	0.95	1.98	1.98	1.98	2.09
15	1.00	1.88	1.98	1.93	1.93
16	1.00	0.51	0.61	0.56	0.56
17	1.45	1.55	1.48	1.52	1.05
18	1.10	1.37	1.32	1.35	1.23

* Assumed to be 1.00. † Probably Abnormal.

The melting points varied from 21.0 to 33.0 degrees, the average being 25.6 degrees. The cause for this wide variation is due in part, no doubt, to different brands of gelatin used, and to possible variations between different lots of the same brand. It is due in part also to different strengths of solution, as shown by the different specific gravities. It is well known, however, that prolonged heating lowers the melting point, and it seems likely that this factor is the most important one. It was found, for instance, that the average melting point of those gelatins which were sterilized in an autoclav was 27.2 degrees, while it was 25.1 degrees in the case of those sterilized in streaming steam for three consecutive days. Yet the gelatin which gave the highest melting point was sterilized on three successive days at 100 degrees. It is to be noted that Samples Nos. 1, 2 and 3, which were tubed when received and which in consequence received one less sterilization than the others, gave the three highest melting points.

SPECIFIC GRAVITY.

The specific gravity was determined at a temperature of 30 degrees by means of a Westphal balance. The observations were made after the gelatins had been heated for several hours so that all were fluid at 30 degrees. The results indicate a considerable variation in the strength of the different samples. It is possible that a slight error may have been introduced by evaporation during the heating, but this could not have been enough to account for the differences in specific gravity observed.

CHLORINE.

The chlorine was determined by titration with silver nitrate, using potassium chromate as the indicator. The results are expressed in per cents. As the amounts of chlorine do not correspond with the specific gravity, it is evident that the amounts of salt present in the different ingredients must be subject to wide fluctuations. The method of determining, however, is subject to error, and these results have only a general interest.

BACTERIAL COUNTS.

The method recommended by the committee was used. The plates were incubated for 48 hours, during which the temperature of the incubator varied between 19 and 20 degrees.

Of the 90 plates, 36 were liquefied wholly or in part. In the case of the plates partially liquefied, the number of bacteria was estimated as accurately as possible. The average melting point of the 36 liquefied plates was 23.6 degrees; of those not liquefied, 28.4 degrees. Brooklyn tap water is a mixture of surface and ground water in about equal proportions. The percentage of liquefying bacteria is low, compared with many surface waters.

CONCLUSIONS.

It is evident that present practice fails to secure uniform results in the preparation of this most important culture medium, even when the operations are conducted in accordance with the rules adopted by the Section at its last meeting. Not only did the gelatins tested vary in strength, as shown by the different specific gravities, but they varied greatly in reaction and in melting point. Their different physical and chemical qualities affected their value for quantitative work to such an extent that when used to determine the number of bacteria in Brooklyn tap water, the maximum number obtained was four times the minimum number.

It would appear from the results obtained that the melting point, or the temperature at which the gelatin becomes soft, has the greatest influence upon the bacterial count. The average number of bacteria found upon the gelatin which had a melting point about 25 degrees C. was 105 per c. c.; the average number found upon gelatin which melted below 25 degrees was 71. This is on account of the rapid spreading of the liquefying colonies in the softer gelatin, with consequent obscuring of the small colonies.

The next important factor appears to be the reaction. If only the gelatins which melted above 25 degrees C. are considered, it is found that the optimum reaction lies not far below 1.5 per cent.—a figure which in previous work has been found to give the maximum numbers with Brooklyn water.

No relation between the count and the strength of the medium or the amount of salt could be observed—and it seems probable that within limits the amount of nutrient material present is of less importance than the melting point and reaction.

The committee is now conducting investigations upon the physical characteristics of gelatins as affected by sterilization and upon the change of reacton during sterilization. These subjects will be reported upon at the next meeting of the section. Any data bearing upon them will be gladly received and duly accredited.

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George W. Fuller, Chairman.

BACILLUS COLI COMMUNIS.

By VERANUS A. MOORE, M. D., Ithaca, N. Y.

Some three years ago I was appointed chairman of a committee to report to this section on the relation of the varieties of the colon bacillus to public health. It seems desirable, after this lapse of time, that a statement should be made concerning the work, although this paper is not to be considered in any sense the findings of the committee, but simply a communication from an individual.

For a number of years I have been deeply interested in the colon group of bacteria. Consequently when this committee was appointed I entered into its work with renewed zeal, hoping that in some way some one would succeed in finding a method for more accurately determining the significance of these organisms from a sanitary point of view. The first undertaking of the chairman was to correspond with each member of the committee concerning a plan of work. It was the unanimous opinion that our knowledge of this group was too meager to warrant, or even to make possible, the preparation of a committee report. It was the belief that for the present individual investigations were most needed to furnish reliable data concerning the biology—the natural history—of this species and its varieties. Since that time a number of important papers have been published on this group. However, instead of a tendency to unify the various opinions and theories already entertained they have carried us into a confusion of results and opinions from which the forthcoming of system and harmony seems more difficult than before.

As each member of the committee took up the particular phase of the subject that appeared to him as most needy of research, or, that from circumstances he could best carry out, my attention became somewhat centered on the boundaries of the species of *B. coli communis* and the extent and the cause of its varieties. At that time the literature of the subject was voluminous and certain well-marked differential characters of the colon bacillus had been formulated. It was known that wells and water supplies were being condemned if the analyst found what he considered to be *B. coli communis* in the water. Pathologists were finding this organism apparently as the etiological factor in a great variety of lesions and bacteriologists were trying to distinguish between extreme varieties of the typhoid and hog cholera bacteria and those of the colon bacillus. Some of the French investigators had gone so far as to suggest that typhoid bacteria were degenerated colon bacilli. More recently a number of investigators have been engaged in working out with great care the characters and properties of certain varieties of this species, and adding many subgroups and sub-varieties to the already almost overtaxed list. There has not, to my knowledge, however, been recorded the results of extended investigations into the phylogeny of the species. It is in connection with this phase of the topic that I desire to approach what seems to me to be the first part of the question before us, namely, what is the colon bacillus? When this organism was first discovered by Emmerisch and designated *B. neopolitans* bacteriological methods were not sufficiently developed to enable one at this time to make even an intelligent guess as to which of the many, at present recognized varieties it belonged. Because of this it seems necessary for us to determine the habitat and to find the limit of the properties that distinguish the species.

As the colon bacillus was first described from the intestine, it is widely believed now that this is its normal abiding place. Because of its hardy nature, however, it is often found in soil and water prevously contaminated with human or animal excrement. It has not to my knowledge been demonstrated in any substance where the possibility of tracing it to this source was precluded. From this point of view the question suggests itself at once, what about the relation of the numerous forms that have been isolated from various extraneous material and classed without challenge among the varieties of the colon group? To my mind the answer to this inquiry involves a knowledge of the as yet undetermined connecting forms between the prototype of the species and the now recognized colon bacillus; or, if its evolutionary origin is not accepted, a better knowledge of the differential characters and properties of the closely allied although distinct species. This brings us at once to the determination of the specific boundaries.

When one begins to fix the characters and properties that shall be considered both diagnostic and sufficient for specific determination there appears an almost endless number of them that claim recognition. However, of these claims, such, for instance, as the curdling of milk within twenty-four hours, the production of indol in certain quantities or any of the many other designated differential properties are carefully weighed there seems to be good reason for not fixing such narrow limits. The fact will appear that if all such definite features are retained they will neutralize each other to such a degree that this species will escape detection. In this connection we are sadly in need of the results of investigations defining the conditions under which such restricted limitations are applicable and under what conditions of life they may vary. That bacteria do change in reference to their cultural manifestations and virulence, if not pathogenesis, has been demonstrated in a number of instances. With this fact which seems to be applicable to all species, can we say that the properties which characterize some cultures—or individuals—shall be considered as diagnostic of the species? For one I protest with the botanists against the fixing of a species on individual characters.

Without entering into a discussion of the literature, it seems to the writer that the results thus far recorded justify the acceptance with slight modifications of the requirements suggested by Smith in 1895 as necessary to distinguish the species. These may be summarized as follows:

A. Morphology.

  1. Slender, motile rods without spores. The motility is often most pronounced and rarely appears only in the organism from young colonies on gelatin plates.

B. Physiology.

  1. The growth of colonies on the surface of neutral or slightly alkaline gelatin is in the form of a delicate bluish, or more opaque whitish expansion with a well defined but irregular margin. The gelatin is not liquefied and the growth is firm.
  2. Milk is coagulated in from one to several days.
  3. The growth upon potato is either a pale or brownish yellow color or merely as a glistening barely recognizable grayish or whitish layer. The quantity and also the color of the growth depends somewhat upon the reaction of the potato.
  4. A distinct but not always marked indol reaction is manifest in cultures 72 hours old. For this, sugar free bouillon is preferable to Dunham's solution.
  5. In bouillon and on agar the growth varies. In bouillon the liquid may become faintly or heavily clouded with or without a surface membrane. On agar the growth may be sparing or more fleshy and vary in color from a neutral gray to a grayish white.
  6. They are divided by their action upon sugars into two groups. First, those that ferment with the formation of gas, dextrose, lactose and saccharose. Second, those that ferment with the formation of gas dextrose and lactose only.

In one per cent. solution in bouillon at 37 degrees C. in fermentation tube:

1. Dextrose, total gas about one-half capacity of closed branch $\frac{H}{CO_2}=\frac{2}{1}$, reaction strongly acid.

2. Lactose, gas production, gas formula, and reaction practically the same as for dextrose.

3. Saccharose, total gas about 2-3 the capacity of the closed bulb, $\frac{H}{CO_2}$ approximately $\frac{3}{2}$ the final reaction may be slightly acid or alkaline, depending upon the time required for the evolution of the gas.

The time required for the production of the maximum quantity of gas varies as a rule from one to three days in case of the dextrose and lactose and from five to twenty days with the saccharose.

C. Pathogenesis.

   1. Many cultures of *B. coli communis* are fatal to guinea pigs of 900 grams weight when inoculated sucutaneously in doses of ½ c. c. of a fresh (24-hour) bouillon culture. Other cultures require the peritoneal injection of a like quantity of the virus for fatal results.
   2. Some cultures do not produce morbid changes when injected in doses of 1 c. c. of the culture into the peritoneal cavity.
   3. Cultures that have been isolated from the intestines of dogs have, in the writer's experience, been more virulent than those obtained from the normal viscera of other animals.
   4. Although the colon bacillus appears to have localized in the digestive tract of living animals (including man) and to that extent parasitic, it is not necessarily virulent as determined by animal inoculation.

If in the study of a culture there should be some deviation from these properties such as the failure to give an indol reaction or to coagulate milk or to act differently in some one or more of the sugars, it does not seem necessary to exclude it from the species. It does not seem necessary either to grant it the distinction of a variety unless it is clearly shown that such deviations are constant. The greater number of variations seem to be found in the effect on milk and sugars, including the gas formula, and in their virulence.

A search for the source of varieties which have been described shows that, with few exceptions, they have been isolated from polluted soil, water, or lesions of various kinds in man or in animals. It seems rational from this to assume as a working hypothesis that at least many of the varieties described from these abnormal habitats of the species are in some way the results of environmental influence. It is along these lines that our subject is begging for experimental results. My study of the colon bacilli suggest that normally, within the digestive tract, there are not many well established varieties. Furthermore, it does not appear from the literature, that there has been as close a discrimination as equity demands between *B. coli* *communis* and other species originally quite as clearly defined. There is a strong tendency to bring all such forms within the limits of the colon group. It is not unlikely that this is the better method, but at present the importance of the colon bacillus as an index to sewage pollution is so great, that I am constrained to believe, that until more definite and experimental knowledge is recorded we should hold its specific characters within or near the limits herein suggested. Forms that are undoubtedly specific varieties should rank in importance equal with the type C species. Those forms that are not distinctively colon as determined by the above mentioned characters can usually be identified as other species or in the absence of such identification they may be placed in the group of undetermined forms existing between the prototype and the species.

NOTES ON B. COLI AND ALLIED FORMS, WITH SPECIAL REFERENCE TO THE NEUTRAL-RED REACTION.

By STEPHEN DeM. GAGE and EARLE B. PHELPS,

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In 1896 the study of the relation of B. coli to the purification of water was begun at the Lawrence Experiment Station. At that time practically nothing was known in regard to the relation of that organism to the public health—either as a possible direct producer of disease or as an indication of the pollution of a water by sewage. Neither was there any reliable procedure by which tests for B. coli could be easily and accurately made. As a result of studies begun by Mr. W. R. Copeland and continued by one of the authors (S. D. G.), on the various characteristics of and tests for the bacillus coli, the procedure known as the "Lawrence Methods" was elaborated, and, in 1898,[1] published as the official method in use at the Lawrence Experiment Station. Since 1898 the results of these tests and their bearing on the interpretation of the analysis of waters which have been filtered through sand, have been published yearly.

Beginning with the application of these tests only to waters which were undergoing filtration, their value as an aid in the determination of fecal pollution has gradually become recognized and they are at the present time applied to samples from a great variety of sources. During the period from 1898 to May, 1902, the routine procedure has been applied at the Experiment Station to 16,000 samples of potable water, 800 samples of ice, 460 samples of shell-fish, 400 samples of sea water, and a considerable number of miscellaneous samples not classified under the foregoing heads. In the same period we have examined culturally nearly 6000 cultures, isolated from these sources. During the earlier years only such records were kept as would be required in a water analysis, showing simply the presence or absence of B. coli in the sample rather than the complete record of the cultural reactions. Beginning in October, 1900, a more complete record of the cultures obtained from the various sources has been attempted. Records have been kept of all the more common characteristics of each culture, and an attempt has been made to isolate and describe every type of culture which varied from what we have become accustomed to regard as the true colon type.

[1] Thirtieth Annual Report Massachusetts Board of Health, 1898, p. 533.

As the species of bacteria present in the samples from the different sources have varied considerably, the records of the tests on the cultures have been grouped according to the source. Under shellfish have been included all cultures from samples of clams, oysters, quahaugs, scallops, etc., and also cultures from samples of sea water which usually accompany these; since many of these samples were suspected of contamination by sewage, we have in them a combination of the sewage and marine flora. In the case of springs, wells and surface waters, only a few cultures were obtained from each, and these are all grouped under one head. Under sewage have been included all cultures from the different kinds of sewage and from the effluents of sewage filters operating at the higher rates, which, in general, appear to pass B. coli more or less readily.

In the following table is shown the summary of the records of the cultures which were studied during the past eighteen months. To avoid confusion the characteristics of the colon bacillus have been included as a heading to the table.

The figures in this table show the percentage value of each test applied to the cultures in the regular work at Lawrence, those in the columns headed + showing the cultures which would have passed that test as B. coli and those in the columns headed 0, the percentage of cultures which would have been thrown out as not agreeing with the colon type. In many cases there is a demand that the analysis shall be made in the shortest possible time even if this means a sacrifice of accuracy. The time may often be shortened by reporting the results of the gelatin tests earlier than on the fourteenth day. The relative accuracy of this test when reported on the fourth, seventh, tenth and fourteenth days, can be roughly estimated from the figures in the table.

THE RELATIVE OCCURRENCE OF B. COLI AND STREPTOCOCCUS[1] FORMS.

During the past few years some trouble has been experienced in making qualitative tests for B. coli, in that occasionally we would find an indication of fermentation, which, when plated out, would give us a pure culture of an organism which did not produce gas in a sugar solution. Winslow and Hunnewell,[2] have referred us to the sewage streptococcus of Houston as an organism which, in mixed culture with B. coli and probably with other fermenting organisms, will, in sugar solutions, overgrow and eventually kill out the fermenting form, after the latter has produced sufficient gas to give the positive preliminary test. During the current year we have made

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[1] Including both streptococci and micrococci.

[2] Science, Vol. XV, No. 386, p. 827.

SOURCE OF CULTURE.	Percent. of Preliminary Fermentations Passing Wurtz Agar Plate.	Agar Streak.					Number of Cultures Inoculated into Confirmatory Media.	Percent. of Cultures.															Percent. of Total Cultures Called B. Coli.
		Number of Cultures made.	Percent. of Cultures.					Fermentation Tube. Dextrose-Broth.		Coagulate Milk.	Reduce Nitrates.	Produce Indol.	Gelatin Stab.										
													Not Liquefied.		Liquefied.								
															Time.				Form.				
			Characteristic.	No Growth.[1]	Dull-Wrinkled.	Viscous.		Produce Gas.	Produce Scum.				Characteristic.	Wrinkled.	4 Days.	7 Days.	10 Days.	14 Days.	Funnel.	Cup.	Stratiform.	
B. coli			+	0	0	0		+	0	+	+	+	+	0	0	0	0	0	0	0	0	
Merrimack River		273	86	0	13	1	236	81	13	86	82	73	69	1	21	25	28	30	10	4	16	59
Filtered Water	76	1485	83	9	4	4	1236	84	3	90	76	71	83	4	4	7	9	13	5	5	3	58
Sewage, etc.		160	87	4	8	1	140	75	0	88	70	75	84	1	9	9	10	15	6	2	7	59
Shellfish	72	312	64	33	2	1	210	68	3	91	67	75	84	0	9	9	9	16	6	2	8	43
Spring Waters	78	53	74	0	4	23	39	56	0	67	67	59	72	0	15	18	28	28	20	0	8	41
Ice	73	79	72	16	8	0	557	67	5	88	42	88	63	7	25	25	26	30	5	4	21	48
Total	75	2362	81	11	5	3	1908	80	4	88	74	71	80	3	8	10	13	17	6	5	6	58

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[1] Including cultures which failed to grow on agar and streptococcus cultures giving a very scanty, non-characteristic growth.

morphological examinations of all such cultures, with the result that the streptococcus forms have in every case been found to be the cause of the anomalous results. The morphological examination is even superfluous in a majority of cases as, after a little experience, one can diagnose the streptococcus forms on the Wurtz agar plate and agar streak cultures, and oftentimes both streptococcus and B. coli can be fished from the same plate. In this work we have made no attempt to examine every preliminary test for the streptococcus form, as we should have done had we accepted this form as an undisputed evidence of fecal pollution. From the literature at hand today we have no positive evidence that streptococcus forms are not to be found outside of polluted material. It certainly is a fact that micrococcus forms in general are abundant in the air and soil. From our own studies we have described no less than seven distinct types of streptococci and micrococci, of which one was obtained from feces, two from shell-fish undoubtedly exposed to pollution, two from filtered water and two from ground water. As far as we know the sources of the last two cultures were in no way exposed to pollution. The acceptance of the streptococcus forms as a direct index of sewage pollution would lead to the modification of the present practice in that we should plate out all preliminary tests, whether or not gas was indicated and examine all cultures thus obtained by the regular staining methods for streptococci.

In general the streptococcus differs from the colon bacillus in the more intense coloring and sour odor on the Wurtz agar plate, due to the greater excess of acid, and in its very small and perfectly round colonies. The colon colonies turn blue within twenty-four hours while the streptococcus colonies retain their red color permanently. The growth on the agar slant is luxurient in the case of the colon while the streptococcus gives a very scant growth, at times only a slight growth in the condensed water at the bottom of the tube being observed. We distinguish on the Wurtz agar plate two distinct types of streptococcus colonies called here respectively Type I and Type II. Type I is of a brick-red color and of such a consistency that the entire colony is readily picked up by the needle point. As a surface colony it spreads slightly, giving the appearance of a halo around the nucleus. Type II produces more acid and is of an intense vermillion color. It forms exceedingly small, non-spreading colonies.

It is our practice to indicate at the time each colony is fished from the Wurtz agar plate, our opinion as to whether the colony is streptococcus, colon, or other form. On the day following a second diagnosis is made in the record book based on the appearance of the growth on the agar slant. As far as they bear on the streptococcus problem, the results of these diagnoses on over one thousand cultures are given in table 2.

From	Diagnosis.			Confirmation.	
		Wurtz Agar Plate.	Agar Slant.	Strepto-coccus.	B. coli.
Water.	Streptococcus { Type I ..	1	1	1	—
	Streptococcus { " II..	61	47	47	9
	B. coli	725	721	4	—
Shellfish.	Streptococcus { Type I ..	20	17	17	2
	Streptococcus { " II..	98	84	84	10
	B. coli	113	106	7	—
Both Classes.	Streptococcus { Type I ..	21	18	18	2
	Streptococcus { " II..	159	131	131	19
	Total	180	149	149	21
	B. coli	838	827	11	—

It will be seen that a rough diagnosis can be made on the Wurtz agar plates, the table showing in this case an accuracy of about 96 per cent. By supplementing this diagnosis with one based on the appearance of the agar slant an accuracy of 100 per cent. is attained in this list of something over one thousand cultures, of which one hundred and sixty were streptococci.

B. COLI AND ALLIED SPECIES.

During the years 1900 and 1901 we have isolated, from the routine studies about one hundred and fifty cultures as worthy of complete species study, and from these we have obtained forty-six cultures which are either different species or different strains of the same species. Some of these are types which would never be confounded with the colon type, while others would be included by various observers within the so-called colon group, from which they differ only in minor characteristics. Of these cultures seven would be classified as belonging to the species B. coli, namely cultures number 49, 50, 56, 58, 67, 70, 86 by the regular Lawrence methods. That they are undoubtedly very closely allied to this species is evident from a study of the cultural reactions, but a detailed study shows that only one of them, namely culture number 70, is, strictly speaking, B. coli, the others differing in one or more minor but important points.

The species characteristics in table 3 were in most cases confirmed by tests made at, at least, three different times and after different periods of storage on media. In each instance tests were made only after an elaborate system of revivification, the various tests being

Group.	Morphology.	Liquefaction.	Dextrose Broth.	Lactose Broth	Saccharose Broth.	Milk.	Nitrate Broth.	Indol.	Nutrient Broth.	Gel. Stab.	Agar Streak.	Potato.
1129	1	2	5	5	5	7	1	1	6	1	4	4
	12	12	3	—	—	7	1235	8	2	1	4	4
	12	12	5	—	—	7	1235	8	2	1	4	4
	12	12	5	—	—	7	4678	1	8	1	4	1
1219	12	2	3	—	—	7	1	1	6	1	4	4
1224	12	1	5	—	—	1	1	1	2	1	4	1
1229	2	1	5	5	5	7	35	1	8	1	4	8
2129	12	1	8	—	—	7	35	1	2	1	4	7
2139	1	1	8	8	8	7	78	8	8	1	6	8
	1	1	8	8	8	7	78	8	8	1	7	7
	1	2	8	8	8	7	78	8	6	1	6	8
	2	1	8	8	8	7	35	1	8	1	4	7
	2	1	8	8	8	7	78	8	8	1	4	7
	12	1	8	—	—	7	78	8	8	1	6	8
2164	12	6	8	—	—	7	78	8	8	4	6	8
2269	1	4	8	8	8	7	78	8	8	4	4	8
2429	1	2	3	3	3	7	78	8	8	1	4	8
	2	1	3	5	5	7	46	8	8	1	4	8
	2	1	5	—	—	7	78	1	8	1	4	8
2439	1	1	8	8	1	7	46	8	8	1	4	8
	1	1	8	8	8	7	46	8	8	1	4	4
	1	1	8	8	8	7	78	8	8	1	6	7
	1	1	8	8	8	7	78	8	8	1	6	7
	1	1	8	8	8	7	78	8	8	1	7	8
	12	2	8	8	8	7	78	8	6	1	4	8
	2	1	8	8	8	7	46	1	8	1	4	8
	2	1	8	8	8	7	78	8	8	1	4	7
	2	1	8	8	5	7	78	8	8	1	4	7
	2	1	8	8	8	7	78	8	8	1	4	7
	4	1	8	8	5	7	46	8	8	1	4	8
2444	2	7	2	5	8	7	35	1	7	2	4	7
2449	1	6	1	8	8	7	8	1	8	2	4	1
2459	1	8	5	3	5	7	78	8	8	3	4	7
	5	8	3	5	8	7	78	1	5	3	4	7
2463	4	4	8	8	8	7	78	8	8	3	4	7
2469	1	4	8	1	1	7	46	8	8	4	6	7
	1	4	8	8	8	7	78	8	8	2	4	8
	1	4	8	8	8	7	78	8	8	4	4	8
	2	4	8	8	8	7	78	1	8	4	4	7
	2	6	8	8	8	7	78	8	8	3	4	8
	4	4	8	8	3	7	78	8	8	4	4	4
	5	7	8	8	8	7	78	1	5	3	4	7
	6	4	8	8	8	7	78	8	8	2	6	7
	8	4	8	—	—	7	78	1	8	2	7	8
	8	8	8	8	8	7	35	8	5	3	7	7
2069	7	4	8	8	8	7	1	1	8	2	4	4

Aerobe.	Wurtz Agar.	Litmus Bleached.	Culture. No.	Source.	Name.
3	8	4	131	Feces.	Sewage Streptococcus (Houston).
4	—	4	88	Shellfish.	Sewage Streptococcus (Houston).
3	—	4	76	Shellfish.	Sewage Streptococcus (Houston).
3	—	4	80	Filtered Water.	Sewage Streptococcus (Houston).
4	—	3	129	Ground Water.	
3	—	4	118	Filtered Water.	
3	8	4	111	Ground Water.	
3	—	2	83	Shellfish.	Bact. anaerogenes (Lembke).
3	2	4	116	Filtered Water.	
3	5	3	110	Ground Water.	
3	8	4	117	Filtered Water.	
3	6	3	136	Shellfish.	Bact. aerogenes (Escherich).
3	8	4	135	Shellfish.	
3	—	4	108	Filtered Water.	
3	—	4	112	Ground Water.	
3	6	4	126	Filtered Water.	
3	1	4	89	Shellfish.	
3	2	4	121	Merrimack River.	
3	—	4	128	Filtered Water.	B. intestinalis (Dyar-Keith).
3	8	3	56	Filtered Water.	
3	8	4	58	Filtered Water.	
3	6	4	51	Merrimack River.	
3	5	4	107	Filtered Water.	
3	1	4	82	Shellfish.	
3	8	4	86	Filtered Water.	
3	6	4	133	Feces.	
3	8	7	50	Filtered Water.	
3	8	4	67	Filtered Water.	
3	8	7	70	Filtered Water.	B. coli (Escherich).
3	8	3	49	Filtered Water.	
3	4	3	120	Pond Water.	
3	1	3	105	Merrimack River Ice.	B. Havaniensis (Sternberg).
3	8	3	71	Filtered Water.	B. hyalinus (Jordan).
3	5	4	125	Filtered Water.	
3	1	3	69	Filtered Water.	B. cuticularis (Tils).
3	8	4	66	Filtered Water.	
3	8	3	60	Filtered Water.	
3	8	3	46	Merrimack River.	B. lactis (Gillebeau).
3	8	3	124	Filtered Water.	
3	8	3	104	Filtered Water.	B. cloasæ (Jordan).
3	8	3	57	Filtered Water.	
3	8	4	130	Filtered Water.	
3	8	4	43	Filtered Water.	B. lactis (Gillebeau).
3	—	3	132	Feces.	
3	6	4	115	Pond Ice.	
3	4	3	127	Filtered Water.	B. dysinterieæ (Kruse).

made each time by a different observer, the same procedure being rigidly followed in each case. The numerical system of grouping and of expressing bacterial reactions, used in this table, is fully described in a separate paper by the writers in this volume under the title, "On the Classification and Identification of Bacteria."

In addition to the tests therein elaborated, the two following columns are added as giving special information of value in the present study:

1. Wurtz agar plates.

   Red colonies, Red colonies later turning blue, Blue colonies.
2. Litmus decolorized in the fermentation tube, with Dextrose, Lactose, Saccharose.

THE NEUTRAL RED REACTION.

The use of the neutral red reaction as a test for B. coli has been proposed so recently and discussed so fully of late that a detailed resumé of its historical aspect will be unnecessary at this time. Since it has been proposed a number of workers have taken up its study and the combined results of their efforts leave us about as well informed on the subject as we were in the beginning. Owing to the numerous and diverse opinions which are held on the subject the writers have undertaken to give the test a fair trial in parallel with the official Lawrence methods. The study has been carried out along two separate lines: First, the value of the test as a method of detecting B. coli in the routine examination of water, and second, a study of the species of water bacteria other than B. coli which give the test.

To carry out the first line of study we prepared a broth as follows:

Meat bouillon	1000 c. c.
Dextrose	5 gms.
Neutral red, 1% sol.	10 c. c.

Reaction neutral to phenolphthalein.

On this broth routine tests to the number of ninety-eight were made in parallel with the regular method. Of the total number of tubes inoculated forty-two gave a positive test, the remainder being negative. All of the negative tests were negative also on the regular dextrose broth, but of the positive tests one gave negative gas tests, and four gave positive gas tests, which further study showed to be due to other bacteria than B. coli. In every case in which the neutral red reaction was given and B. coli not found in the regular test, examination of the neutral red tube showed that the reacting organism was not the colon bacillus as we interpret it.

On the second line of study mentioned, namely the study of species other than B. coli which give the reaction we have proceeded as follows:

Species selected from our stock species were revivified in the usual manner and used as test species on the neutral red media. These species were selected so that, as far as possible, each numerical group of the appended table would be represented. A brief description of the methods and results of the test with these various species will be given.

Neutral red agar was made up as follows:

Meat bouillon	1000 c. c.
Agar	10 gms.
Peptone (Witte)	10 gms.
Dextrose	3 gms.
Neutral red, 1% solution in water	10 c. c.

On this agar the cultures named below were plated out and grown at 37° C. The results are shown in the following table, in which the plus signs indicate a change from red to yellow, and the zeros no change:

		Series No. 1.	Series No. 2.	
Group.	Culture.	24 hours.	15 hours.	40 hours.
1129	131	+	0	+
1229	111	+	0	+
2139	135	+	+	+
2139	136	+	+	+
2269	126	+	+	+
2429	121	+	+	+
2429	128	+	+	+
2439	133	+	+	+
2449	105	+	+	+
2459	125	+	+	+
2459	71	+	+	+
2463	69	+	+	+
2469	115	0	0	+
2469	132	0	0	+
2469	43	+	+	+
2469	46	+	+	+
2469	104	+	0	+
2069	127	0	0	0[1]

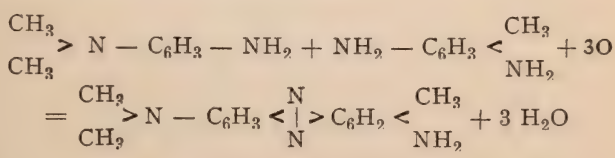
[1] Yellow in 64 hours.

On the same agar the same species were used in making shake cultures as recommended by Buxton. The results at various stages of the growth are shown in the following table:

Group	Culture	Gas	Amount of yellow produced. Entire tube = 100.					
			15	20	48	72	96	168 hours.
1129	131	0	slightly orange.					brown.
1229	111	+	50	100	—	—	—	yellow bottom, brown top.
2139	136	0					30	brown.
2139	135	+	50	50	100	—	—	yellow bottom, brown top.
2269	126	+			50	100	—	" " " "
2429	128	0	10	20	30	30	30	50 turning brown.
2429	121	0					trace	orange.
2439	133	+	50	100	—	—	—	yellow bottom, brown top.
2449	105	+			100	—	—	" " " "
2459	71	0				10	50	reddish brown.
2459	125	0				30	50	brown. Darker on top.
2463	69	+			100	—	—	yellowish brown.
2469	43	+			100	—	—	brown.
2469	46	+			50	100	—	"
2469	132	+				100	—	yellow bottom, brown top.
2469	104	+			100	—	—	" " " "
2069	127	0	slightly orange.					

Lack of time has prevented our applying the test to all of our stock cultures. From the results of the tests which we have made, however, we are inclined to believe that the great majority of our cultures, if indeed not all of them, will give this reaction. As has been stated the cultures were selected to represent each one of our numerical groups. Of the groups which were omitted we have no representative in stock. The reactions on cultures number 131, 121, and 127 would be called doubtful in the shake culture, although on the plate culture the test was given in every case.

Concerning the chemistry of the reaction very little has been written as indeed very little seems to be known. Neutral red or toluylene red is prepared from para-amido-dimethyl-aniline and meta-toluylene-diamine by direct oxydation as follows:



$$\begin{matrix} CH_3 \\ CH_3 \end{matrix} > N - C_6H_3 - NH_2 + NH_2 - C_6H_3 < \begin{matrix} CH_3 \\ NH_2 \end{matrix} + 3O$$

$$= \begin{matrix} CH_3 \\ CH_3 \end{matrix} > N - C_6H_3 < \begin{matrix} N \\ | \\ N \end{matrix} > C_6H_2 < \begin{matrix} CH_3 \\ NH_2 \end{matrix} + 3\ H_2O$$

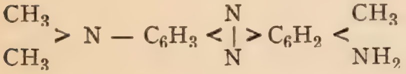
From the mode of its formation neutral red is seen to be a member of the saffranine group of dyes, having the typical saffranine chromophore and reacting like saffranine under the influence of reducing agents.

The production of the yellow color has been variously ascribed to one or two causes. Since on addition of alkali to an aqueous solution of the dye it changes color from red to yellow, it has been frequently assumed that the yellow thus produced and the yellow fluorescing dye produced by the colon and other bacteria, are identical. That such is not the case is plainly evident from the fact that the medium in which the bacteria produce the yellow color is strongly acid (about 5 per cent.), and that the yellow produced by alkali is not fluorescent. The second explanation is less definite in its claims and merely assumes that the color change is the result of a "reduction." That such a view is erroneous, if reduction is used in its true chemical sense, is made manifest by a chemical study of the dye. The first reduction possible in this case is to a dye known as toluylene blue, an indamine body. Further reduction produces a complex amido body in which the two benzene rings are held together by only one nitrogen atom, while a continued reduction causes a separation of the two rings into the two amido bodies from which the dye is made. The only one of these substances with which the yellow coloring matter might be confused is the second product, a pale yellow amido compound. We have prepared this substance and find that it differs from the bacterial product in the following points:

1. It is not a dye. The yellow color produced by the colon bacillus is a powerful dye, dyeing unmordanted wool almost instantly in the cold.

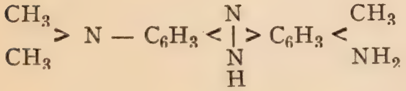
2. On exposure to the air it rapidly oxidizes to neutral red. The yellow dye oxidizes only very slowly and then to a brown substance insoluble in water.

The relation of these various products is best shown in the structural formula given below.



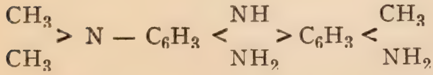
$$\begin{matrix}CH_3\\CH_3\end{matrix} > N - C_6H_3 < \begin{matrix}N\\|\\N\end{matrix} > C_6H_2 < \begin{matrix}CH_3\\NH_2\end{matrix}$$

Neutral red or toluylene red



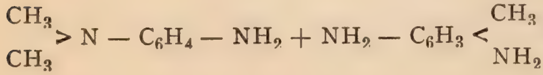
$$\begin{matrix}CH_3\\CH_3\end{matrix} > N - C_6H_3 < \begin{matrix}N\\|\\N\\H\end{matrix} > C_6H_3 < \begin{matrix}CH_3\\NH_2\end{matrix}$$

Toluylene blue.



$$\begin{matrix}CH_3\\CH_3\end{matrix} > N - C_6H_3 < \begin{matrix}NH\\ \\NH_2\end{matrix} > C_6H_3 < \begin{matrix}CH_3\\NH_2\end{matrix}$$

Yellow amido body.



$$\begin{matrix}CH_3\\CH_3\end{matrix} > N - C_6H_4 - NH_2 + NH_2 - C_6H_3 < \begin{matrix}CH_3\\NH_2\end{matrix}$$

p-amido-dimethyl-anilin and m-toluylene diamine.

We are not prepared at this time to state just what the true nature of this color change is, but studies are now under way which we hope will clear up this question. From what we have thus far learned we believe that the yellow color will be found to be due to a dye of an entirely different series, possibly of the fluorescein series of colors. If such be the case, and all the evidence points that way at present, we have opened up a very interesting field of speculation as to how the change has taken place. In the saffranine group we have essentially two benzene rings held together by two nitrogen atoms, while in the fluorescein group the rings are held together by one bonding atom of carbon.

In our attempts to study the chemical nature of the change we have used a synthetic medium proposed by DeSchweinitz,[1] for such studies. This medium is composed of distilled water in which are dissolved the following:

$MgSO_4$	0.02%
K_2HPO_4	0.10%
$(NH_4)PO_4$	1.00%
Glycerine	4.00%
Neutral red, 1% sol.	1.00%

In this medium the reaction is given fully as well as in the other media proposed, and the products are more readily separated for study. Much to our surprise we obtained good fermentations in the case of many of our species.

The return of the red color noted by Buxton has been observed and studied. Our experience has been that the color is not the same red as the original, but more of a brownish red. Such a change can be brought about in an aqueous solution of the yellow coloring matter by adding a slight excess of hydrochloric acid, or simply by allowing the solution to stand for a few days exposed to the air. We are inclined to think that both an excess of acid and a slow oxidation of the media in the shake culture combine to give this so-called return of the red color. The color is not neutral red, but a dye of unknown structure, dyeing wool an old rose.

CONCLUSION.

Although the study here reported is necessarily incomplete, we think that we are justified in the following conclusions:

Owing to the large number of bacterial species giving the neutral red reaction it has little or no value as a confirmatory test. Its value in this respect is lessened by the fact that those species giving the preliminary colon tests, and from which we wish to separate the colon bacillus by confirmatory tests, give the reaction almost without exception. Its usefulness therefore will be as a preliminary test if it is to be used at all.

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[1] Culture Media for Bio-chemic Investigations, E. A. De Schweinitz, Sixteenth Annual Report of the Bureau of Animal Industry, U. S. Department of Agriculture, p. 145.

Judging from our own work as well as from the work of others, we believe that as a preliminary test for B. coli this reaction will be found to be as sensitive and as accurate as any which has been proposed. In the very limited number of tests which we have made (we have tested only about one hundred waters), we have found the neutral red tests to agree with our regular dextrose broth tests as far as positive reactions were concerned. In every case in which B. coli was present it was shown by both tests. The neutral red reaction has, however, given positive reactions in which the reacting organisms were other than B. coli. The two tests being equally accurate for B. coli, that test will be found the most useful which will give positive reactions with the smallest number of other species. In this respect the neutral red test is seen to be much inferior to our regular dextrose broth method. In our culture studies we have shown that all of the stock cultures which we have tested gave a positive reaction with neutral red. To be sure these are selected cultures, but, on the other hand, they are the cultures which we have picked out from several thousand preliminary tests as the very cultures which will be most often met in routine B. coli tests and from which the colon bacillus is differentiated with the most difficulty. About one-half of the cultures tested are non-gas formers and would be eliminated at once by the gas test.

We have confirmed in general the observations of previous workers, notably those of Buxton,[1] Irons[2] and Savage.[3] We dissent, however, from the conclusions of the latter paper, in which any non-liquefying organism, giving the neutral red reaction and either the milk, indol, or gas test, with a turbidity in broth is called B. coli. Such a broad definition of this species is commonly seen in the work of English writers and it is easily seen how, with such a criterion, the neutral red reaction has been found almost specific for B. coli. We believe, that with such a broad criterion, the entire significance of B. coli is lost. Eight of the seventeen cultures shown in table 4, cultures selected as representative of our numerical groups, would pass Savage's test for B. coli. While we have not tried the neutral red reaction on any others of our stock cultures, the results of our work thus far lead us to believe that the majority of our cultures would give the reaction. If such is the case, we would have twenty-seven distinct species, in fact all of our non-liquefyers but one, passing the test for B. coli.

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[1] A comparative study of the bacilli intermediate between B. coli communis and B. typhosus. B. H. Buxton, Jour. Med. Res., III (n. s.), p. 201.

[2] Neutral red in the routine examination of water. E. E. Irons, Jour. of Hygiene, II, 314.

[3] The examination of water for the bacillus coli communis. W. G. Savage, Public Health, XIV, p. 485.

ON CERTAIN PRECAUTIONS REQUIRED IN MAKING AND INTERPRETING THE SO-CALLED "COLON TEST" FOR POTABLE WATERS.

By S. C. PRESCOTT, Boston, Mass.

Although the literature upon bacillus coli is copious, it is only recently that it has been definitely established that this organism very closely resembles some of the most common bacteria occurring in nature and the arts. Numerous investigators have claimed that it may be isolated from waters which are apparently free from recent pollution, and in 1894 Kruse even went so far as to state that "the bacterium coli is in no way characteristic of the fæces of men or animals. Such bacteria occur everywhere, in air, in earth and in the water, from the most different sources." Kruse, however, offered no experimental data to support his assertion.

In a brief preliminary paper presented by me at the last meeting of the Society of American Bacteriologists, at Chicago, I announced, as the result of a detailed study of certain lactic-acid bacteria isolated from sources apparently free from contamination with fæcal matter, the striking similarity of their cultural reactions to those of B. coli. Since giving my Chicago paper I have repeated and extended my work and the results have led me to believe that these organisms are not merely similar in certain reactions, but absolutely identical. I desire at present to consider the bearing which these facts may have upon the interpretation of the results of sanitary bacteriological water analysis, and particularly to point out some precautions which must be observed in securing the data indispensable for, and in reporting the results of, such analyses.

At the outset I may explain what I mean by "colon bacillus" and "lactic acid bacteria." We are accustomed to regard the colon bacillus as an organism of intestinal origin, having the following morphological and physiological characteristics:

A short, usually motile rod, with diameter generally less than one micron, and having no spore formation. It forms thin, irregular films upon the surface of gelatin, produces no liquefaction, and gives a wire-nail like growth in stick cultures. It forms a white translucent layer of charactertistic appearance upon agar; produces a more or less abundant moist yellowish growth on potato, turbidity and some sediment in broth, ferments dextrose and lactose, and generally saccharose, with formation of gas, and reductes nitrates to nitrites. It coagulates casein in litmus milk and reduces the litmus with subsequent slow return of the color, and forms indol in peptone solution.

The lactic-acid group is harder to define because the name is used in a broader and looser way. Many fermentation physiologists regard all those organisms in which the ability to ferment sugars to lactic acid constitutes a predominant or strongly characteristic property as members of the lactic-acid group and it is with this meaning that I employ the term. On this basis B. coli and many others must be regarded as true lactic-acid bacteria.

The microörganisms which I studied were isolated from various sources, but chiefly from grains and products of milling such as flour, bran, cornmeal, oats, etc., and corresponded in most respects to the rather meagre published descriptions of the *B. acid lactici* of Hueppe. There is no evidence that any of these organisms were of intestinal origin. Of forty-seven cultures, twenty-five gave all the reactions regarded as typical of B. coli, while several others failed only in a single test, thus corresponding to the so-called para-colon. I do not dwell upon the lactic-acid bacteria from milk, since these are so likely to be of direct intestinal origin that they would shed no light on the question at stake.

From the sanitary point of view the results of the research which I have described seem to be of practical importance, inasmuch as at the present time special significance is generally attached to the colon test, and no bacteriological examination of drinking water is to-day regarded as complete without it. While the rough quantitative determination of bacteria may supply much useful information as to the character of a water, it is generally regarded that pollution or purity may be more definitely determined by the positive or negative character of the much used special tests for B. coli.

Probably none of the bacteria has been studied with greater zeal during the past ten years than the colon bacillus. Having been found to be a constant inhabitant of the intestine of man and many other animals, it is a widely accepted theory that the intestine is its natural habitat, and that its occurrence elsewhere indicates recent pollution from fæcal sources. It is therefore an organism which has been and must be regarded as of great sanitary significance and it is from this point of view that the excellent series of cultural reactions which constitute the colon test has been developed. Since, however, it has now been definitely proved that many so-called lactic-acid bacteria will give exactly the same reactions, we are led to the conclusion that a positive colon test does not necessarily indicate direct intestinal origin. What, then, does a colon test prove? Obviously (1), the presence of a colon bacillus which has come *directly* from the intestine of some animal, or (2) of a colon bacillus which has come indirectly from an animal intestine, or (3) of a "lactic-acid bacillus" so-called. This immediately raises the interesting questions, is it possible that the lactic-acid bacilli have been indirectly derived from animal intestines, having "escaped from cultivation," as the botanists say? Or is the converse true, namely, that all colon bacilli are simply lactic-acid bacteria which have found in the warm intestinal contents richly supplied with food a favorable habitat?

The answer to these questions is of much theoretical interest, but need not be further considered here. What we really want is to be able to decide whether the bacteria we have found, no matter what they may be, have or have not come *recently* from an animal intestine. It may be urged that while the matter is of great theoretical importance it has no practical significance, for even if colon bacilli do exist abundantly, in refuse from lactic-acid factories, tanneries, grist mills or dairies as well as in animal intestines, they still mean wastes and drainage. It is quite conceivable, however, that B. coli originally derived from intestinal matter, but which has persisted for a long time in nature and possibly has even flourished there, may find access to potable waters, and in such cases the positive colon test would mean but little since it is *recent* rather than *remote* pollution which is dangerous, and which it is of most importance to detect.

It is also conceivable that such "escaped" organisms might gain entrance to water without being introduced with refuse of any kind, since they are now shown to be so widely distributed in nature. For example, we might reasonably expect a stream flowing through large grain fields to receive considerable numbers of such bacteria, and examination of these waters in the usual way for B. coli would very likely in many cases give positive reactions. Judged then by mere presence or absence of colon bacilli waters might unquestionably be reported as sewage-polluted, when, as a matter of fact, they had received no fæcal contamination whatever. To what extent this is true in fact is as yet a matter of conjecture. The presence of colon bacilli in greater or less numbers in waters apparently absolutely free from contamination has been reported by various investigators, and these results may perhaps be explained as here suggested. Thus to mention only two, Poujol in 1897, reported the isolation of B. coli from 22 out of 34 waters, springs, well and rivers, all of fair quality and many quite free from any possibility of contamination; and Weissenfeld later obtained positive tests from B. coli from thirty samples of water considered to be pure. It should be stated, however, that Weissenfeld did not adhere strictly to all modern requirements in identifying B. coli. My results in any event do not invalidate the colon test, but simply make plain the necessity for special care and judgment in its application and in the interpretation of results. They strongly support the statement made by Dr. Theobald Smith in 1892, and later by Freudenreich and a few other investigators, that it is the *number* of colon bacilli present in a water rather than the presence of this organism which should be considered in deciding whether or not recent sewage pollution of the water has taken place. Judging from the results obtained by my friend and colleague, Mr. C.-E. A. Winslow, who has carefully studied a large number of polluted and unpolluted waters in Massachusetts, and whose paper on that subject will shortly be published, the presence of B. coli in one cubic centimeter of the suspected water in a large proportion of cases is required in order to establish reasonable proof of sewage pollution. It may be necessary to modify this standard somewhat for other, and more agricultural, parts of the country, but the important point is that if the colon bacillus is found only rarely, or only in large samples of water, its presence has no great sanitary significance.

Since our analysis for B. coli must, then, be quantitative as well as qualitative, the method used in qualitative determinations becomes of special importance. Two methods are in common use, one in which the litmus-lactose-agar plate is used as a starting point; the other, in which a preliminary cultivation is made in dextrose broth in the fermentation tube. In the plate method, the probable presence of colon bacilli is indicated if, after growth for 24 hours at 37°, red colonies are developed upon the plate. From each of these colonies dextrose broth is then inoculated and allowed to grow for 24 hours at body temperature, when if gas is present other special tests for B. coli may be made. In some laboratories it is the custom to regard this method as quantitative as well as qualitative and to report *all* red colonies as B. coli, a procedure which is obviously not justified, since many bacteria may ferment lactose with formation of acid. In my opinion the error thus introduced far outweighs the advantage gained by enumeration, for it is the relative, rather than the absolute, number of colon bacilli that we wish to discover.

The second method, that in which the preliminary fermentation tube is employed, is generally used as a qualitative test only, although it may be so modified as to be of great quantitative value (as suggested by Dr. Smith) by using a large number of tubes and inoculating each with a fractional part of a cubic centimeter of the water examined. This offers an easy, and at the same time a fairly reliable, method for estimating the approximate number of colon bacilli. For example, if each tube of one series be inoculated with 1-10 c. c. of water, and each tube of another series with 1-100 c. c. and if results are positive in a majority of the former but in very few of the latter series, we should conclude that the number of colon bacilli is not far from 10, and much less than 100 per cubic centimeter.

The precaution most necessary to take in applying this method qualitatively is to guard against too long incubation of the preliminary fermentation tube. The duration of this incubation is generally 24 hours, at 37°, and while this is undoubtedly not too long for waters containing few colon bacilli, it may be too long for highly polluted waters, because of the development of streptococci which may overgrow the B. coli, and either kill or weaken them by producing lactic-acid in greater amounts than the colon bacilli can endure, with the result that later tests may entirely fail to reveal the presence of any colon bacilli.

The discovery of the streptococci in sewage and sewage-polluted waters in England by Houston and subsequently in America by Winslow must be regarded as of great significance in sanitary bacteriology, for it seems likely that the detection of these organisms in water will offer a means of confirming or strengthening the evidence offered by the colon test. It is possible, on the other hand, that they, like colon bacilli, may be widely distributed as saprophytic bacteria on vegetation and in the soil. Their presence in cultures from grains has been observed, but to what extent they may occur outside of sewage I am unprepared to state, and until it is shown that the streptococci are as characteristic of sewage as are colon bacilli, and as important in indicating fæcal contamination, careful precautions must be taken to prevent their interference with the colon test.

I may recapitulate as follows:

1. Bacteria corresponding in every way to B. coli are by no means confined to animal intestines, but are widely distributed elsewhere in nature.

2. The finding of a few colonies of B. coli in large samples of water, or its occasional discovery in small samples, does not necessarily have any special sanitary significance.

3. The detection of B. coli in a large proportion of small samples (1 c. c. or less) examined, is imperatively required as an indication of *recent* sewage pollution.

4. The *number* of colon bacilli in water rather than their *presence* should be used as a criterion of *recent* sewage pollution.

5. Inasmuch as some red colonies other than those of B. coli appear on a litmus-lactose-agar plate, this test alone cannot be regarded as diagnostic.

6. In tests for the presence of B. coli by the usual methods the possible inhibitory effect of lactic-acid-producing streptococci must not be overlooked.

THE COLON BACILLUS IN GROUND WATERS.

By ELMER G. HORTON, B. S.,

Bacteriologist and Chemist to the Ohio State Board of Health..

(From the Laboratory of the Ohio State Board of Health.)

During the past four years it has been part of our routine work to examine samples of ground waters from wells and springs in various portions of Ohio, and, in so doing, organisms have been obtained from time to time conforming to the tests usually given as differentiating fecal bacteria.

The mere finding of B. coli in ground waters is not new, as its presence in several cases has been mentioned by other workers, but we desire especially to call attention to the significance of the presence of B. coli in ground waters, *i. e.* ground waters derived from springs or drilled wells.

Naturally there have been many samples from dug wells in the waters of which the colon bacillus was present, but in the present article we omit all reference to such samples, and include only those derived from springs and drilled or bored wells that at the same time contained B. coli. The list includes 25 samples from wells varying in depth from 16 to 243 feet, although most of the wells were from 25 to 80 feet deep. Almost without exception these wells were cased and with the casing extending above the level of the ground, thus preventing the direct entrance of surface water. The list also shows 12 samples of water from springs in which colon was found. Part of the springs were open to surface pollution, while others appear to have been well protected.

With a few of the samples the analysis was bacteriological only, but generally there was also a more or less complete chemical examination. The number of bacteria per c. c. varied from 16 to 900,000 in the well waters, and from 90 to 400,000 in the samples from the springs. Some of the chemical and bacterial analyses gave marked evidence of sewage pollution, indicating a somewhat open course from some sewage source to the water-producing strata in question. In other cases the chemical evidence of pollution was small, while in still others, there was no chemical evidence of sewage pollution and the waters would have been passed without question had there been only a chemical analysis.

Since some of the waters under examination were those proposed as public supplies for villages or cities, and some were from localities where typhoid fever was or had been present, we did not feel warranted, at the present time, in accepting as final any of the gross, or preliminary, or presumptive tests, although such were made use of it in connection with the more laborious isolation and identification by cultivation on various media, etc. The final conclusion depended almost entirely on the results obtained by the latter process.

During the period of this study different methods have been resorted to for the isolation, and much of the time two or more methods were employed simultaneously. The methods have been as follows:

(*a*) Preliminary cultivation in Dunham's peptone using 40 or 90 c. c. of the water in question, and followed by plating to lactose-litmus-agar (or in a few cases to gelatin) from which colonies were fished and subcultivations made.

(*b*) Cultivation in glucose broth using 40 to 90 c. c. of water with subsequent plating, etc.

(*c*) Same as *b* except that 1 c. c. of the water was used.

(*d*) Cultivation in carbolated sugar broth using 40 or 90 c. c. of water with subsequent plating, etc.

(*e*) Same as *d* except that 1 c. c. of water was used.

After plating, it was customary to fish from four to six colonies and test as follows: Morphology, motility, litmus milk, nitrate solution, peptone for indol, neutral red, gelatin stab, gas production in sugar broth, gas formula of 2:1. A portion of the above individual tests were omitted with some of the samples, but not with the more particular ones.

In view of the work by Prescott on colon and allied forms, the question arises as to whether the organisms isolated were colon bacilli, and, accordingly, other than bacteriological evidence was sought in the history of these waters in relation to typhoid fever among the users. Of the thirty-seven samples, twenty-seven gave a history of typhoid fever, the number of cases per well ranging from one to sixteen, and from one to seventeen for the springs. Of the remaining samples two were from springs that could hardly escape animal pollution. With the evidence at hand we feel warranted in saying the organisms isolated were colon bacilli, and that the presence of this organism is at present to be looked upon in spring waters with grave suspicion, unless there is known opportunity for surface pollution carrying waste material from the lower animals. The results obtained lead us to believe that the presence of B. coli in waters from drilled wells ought in most cases to condemn the use of that water for domestic purposes.

An exceptional case may be stated here. A drilled well thirty-five feet in depth was put down for the proposed supply of a village near the central portion of the state. On examining the water the more important chemical findings were as follows, in parts per million: Oxygen consumed, .46; N as albuminoid ammonia, .039; N as free ammonia, .014; nitrites, none; nitrates, trace; chlorine, 33.1. (It should be stated that the chlorine is not a close indicator in many parts of Ohio owing to the presence of salt.) The number of bacteria per c. c. was eighteen. In the qualitative bacterial examination, each of the above individual tests was applied to the organisms isolated with positive results, *i. e.*, B. coli was present. A second sample a month later gave the same organism, but in view of the excellent chemical analysis, the low number of bacteria, and the unqualified recommendation of the civil engineer of the State Board of Health, who had made a personal inspection of the locality, the sample was allowed to pass. Subsequently an examination was desired, but unfortunately the portion for bacterial examination was not taken by the collector, and only a chemical analysis was made. This analysis showed even better results than the former one.

CONCLUSIONS.

1. We believe the organism found in these waters from springs and drilled wells was B. coli.
2. The presence of B. coli in spring water should be looked upon with suspicion, unless there is apparent opportunity for pollution by the lower animals.
3. The presence of B. coli in water from drilled wells should generally condemn that water for domestic use.
4. The fact that a water is derived from a drilled well should not be taken as an absolute guarantee that it is a potable water.
5. Both chemical and bacterial examinations of a water are desirable, but the bacterial will sometimes prove the more delicate indicator of direct pollution which is the more dangerous pollution.

ON THE PRACTICAL VALUE OF PRESUMPTIVE TESTS FOR BACILLUS COLI IN WATER.

By GEORGE C. WHIPPLE, of Brooklyn, N. Y.

The status of the test for Bacillus coli in the sanitary examination of drinking waters is, at the present time, one of great uncertainty, and yet there is no subject of more practical importance. This uncertainty is caused in part by the fact that there is no one method generally accepted by which the presence of this organism in water is determined. Each bacteriologist has his own way of isolating it and studying it in pure culture, and the results obtained by these different methods fail to agree. The bacillus itself, in spite of an immense amount of study, is one which cannot be always recognized by the usual methods employed. Its different races and strains so insensibly merge into allied species, and there are such gradations between Bacillus coli and Bacillus typhi on the one hand and between Bacillus coli and several species of common water bacteria on the other hand, that with our present tests it is almost impossible to say what shall or what shall not be called Bacillus coli. The type form of Bacillus coli is one which can be defined within reasonably narrow limits, but when the organism has been away from its natural habitat for varying periods of time, and has existed under abnormal conditions, its ability to react normally to the usual tests is greatly impaired. Its power to reduce nitrates may be lost, or, on the other hand, it may be much increased; its power to produce indol may be lost, or, on the other hand, it may be increased; its power to coagulate milk, even, is sometimes reduced, although seldom entirely lost; its power to ferment carbohydrates may be altered so that the amount of gas obtained in a fermentation tube, as well as its ratio of H to CO_2, is quite abnormal. But in spite of all these facts the bacillus tested may have been originally a true Bacillus coli. It happens, therefore, that a sample of water may contain Bacillus coli without its being recognized, and it happens also, and perhaps more frequently, that other organisms closely related to Bacillus coli but different from it, may be recorded as Bacillus coli. And this has led to a wide divergence of ideas concerning the distribution of Bacillus coli in nature and to the practical significance of positive tests for Bacillus coli in water. So great has been the discord in regard to the results obtained by the usual tests for Bacillus coli that it has been questioned by some sanitarians as to whether the test as ordinarily made has any practical significance at all. It is my purpose in this article to show that in spite of all these varying results the test for Bacillus coli is one of great usefulness.

In order to determine definitely whether B. coli is present or is not present in a sample of water, it is necessary to isolate the germ and study it in pure culture. This involves a long series of operations, and is a time-consuming process. It is seldom that a reliable result can be obtained inside of one week, and preferably the test should extend over a period of ten days, in order to allow for preliminary cultivation.

The following tests may be considered necessary to establish beyond a doubt the identity of the organism:

1. Typical morphological characteristics and motility, as determined from a young broth culture.
2. The non-liquefaction of gelatin as determined by a stab-culture incubated for at least one week, and preferably for a longer period.
3. The fermentation of dextrose broth in a Smith tube. The amount of the gas should be normally between 30 and 70 per cent. of the volume of the closed arm, and the per cent. which the carbonic acid is of the total amount of gas should be approximately one-third. The closed arm should be turbid and the open bulb should show a strongly acid reaction. It should be noted, however, that both the amount of gas and the percentage of carbonic acid are subject to variation, even with freshly isolated cultures of B. coli, so that these quantities must not be given a too strict interpretation.
4. The production of indol in Dunham's solution, when tested by the "Ring Method" after an incubation of 48 hours at 37 degrees C.
5. The characteristic coagulation of milk, with the production of acid, in 48 hours at 37 degrees C.
6. The reduction of nitrates in 48 hours at 37 degrees C.
7. The non-formation of spores, as determined by the inability of a culture to develop after heating for 10 minutes at 80 degrees C.

The most important of the above tests are the characteristic morphology, non-liquefaction of gelatin, fermentation of carbo-hydrates and the coagulation of milk. The tests for indol production and nitrate reduction are somewhat more erratic than the others. Before the above tests can be made, it is first necessary to isolate the organism from the water in pure culture. This has been accomplished in a variety of ways. For example: Gelatin plates, agar plates, lactose-litmus-agar plates, phenol-agar plates, or plates of agar which have been otherwise treated in order to inhibit the growth of ordinary water bacteria, are fished, and the cultures put through the regular tests. Or, the sample is added to dextrose broth in a

Smith tube, and, if the characteristic fermentation is obtained, thereafter plated out on agar or some modification of it, as described above, and these plates ultimately fished. Or, the sample of water in quantities which may vary from .01 c.c. to 1 litre, is enriched by the addition of carbolized dextrose broth, or some similar medium, and given a preliminary incubation, after which transfers are made to lactose-litmus plates or to the fermentation tube. In fact the methods used for isolating B. coli in pure culture are varied, according to the idiosyncrasies of the operator and the character of the water to be tested. It is not my purpose to describe here these various methods, many of which are of considerable value, and no one of which can be considered as superior to the others for all cases. I wish simply to emphasize the fact that the operation is a tedious and laborious one. In the practical supervision of water supplies it often happens that there is no time for carrying out all of the required tests to obtain an absolutely conclusive result, and that an approximate test which may be quickly made may have greater practical value than the ultimate test. For example: If it only takes 4 days for the water of a reservoir to reach the service taps in a city, and if it requires a week to make a test, the result of a conclusive test cannot be learned until several days after the water had been used, while if a presumptive test could be obtained in 48 hours it might materially affect the management of the water supply. Hence, it is important that the subject of presumptive tests for the presence of B. coli should not be overlooked. I have studied this subject with considerable care, and have made practical tests of nearly all methods which have been suggested, including the most recent ones, namely, the neutral red test and the sodium-taurocholate, or bile salt test, as suggested by McConkey,[1] but after all my studies *I have found nothing of more practical value than the simple fermentation tube test, as first outlined by Smith;* and his original test has been modified only by using different volumes of each sample of water. The method of procedure which is used at Mount Prospect Laboratory is as follows:

Smith tubes of the ordinary form are used, the closed arm being five inches long and about one-half inch inside diameter.

The following is the formula of the broth used:

Liebig's Beef Extract	10	grams
Pepton	10	"
Dextrose	10	"
Water	1000	"

The reaction is made 0 per cent. and the correctness of this reaction is a matter of no little importance.

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[1] In a later communication I hope to be able to present the detailed results of some of these tests.

The medium is made up in batches of 5 or 10 litres and kept in flasks until needed to be used in the Smith tubes. Sterilization is made in the autoclav. When quantities of water of .001, .01, and 10 c.c. are to be tested, these quantities are added directly to the broth in the Smith tubes. When larger quantities such as 100, 500 or 1,000 c.c. are to be tested, they are first enriched by the addition of carbolic dextrose broth of such a strength that after its addition to the water the mixture shall contain 1 per cent. of dextrose and 0.1 per cent. of phenol. After preliminary incubation for a period not exceeding 12 hours, 1 c. c. of the culture is added to a fermentation tube. If the enriched samples are allowed to stand too long other organisms may outgrow B. coli so that a negative result may be obtained. The fermentation tubes are incubated at 40 degrees for 48 hours, and are then tested for per cent. of gas, per cent. of carbonic acid, reaction of the open bulb, turbidity of closed arm, and presence of a scum on the open arm. The typical reaction which is considered as a positive test for the presence of B. coli in the sample tested is as follows: Total gas, between 25 and 70 per cent. of the closed arm; per cent. which the carbonic acid is of the total gas, between 25 and 40; the closed arm turbid; the open arm strongly acid to litmus and usually accompanied by the non-formation of a scum. If the amount of gas formed is between 10 and 25 per cent., or if it is more than 70 per cent., the test is considered as atypical; or, if the amount of gas is between 25 and 70 per cent., but with the per cent. of carbonic acid greater than 40, the test is considered as atypical; or, with the proper gas reactions, but with the open arm alkaline instead of acid, the test is considered as atypical. If no gas forms, or if it is less than 10 per cent. the test is called negative.

It is recognized that this test is not absolute. It has frequently happened that pure cultures isolated from Smith tubes which gave a positive test have failed to yield true cultures of B. coli when carried through all the tests by sub-culture; and on the other hand it has occasionally happened, though rarely, that true cultures of B. coli have been obtained from Smith tubes which yielded atypical results. But speaking broadly, the typical reactions offer satisfactory presumptive evidence of the presence of B. coli, or species very closely allied to it.

In a subsequent paper I hope to give statements to support this assertion, but at this time will offer as corroborative testimony the statement of Mr. Irons, who, in his paper on "Some Observations on Methods for the Detection of B. coli communis in Water," states: "When the dextrose tube yields approximately 33 per cent. of CO_2 B. coli communis is almost invariably present."

As an illustration of the value of this test, I beg to call attention to the diagram shown in Figure 1. This gives the results of chemical and bacteriological examinations of certain surface water supplies classified in six groups, according to their relative pollution, as determined by inspection of the watersheds. Each group represents several different streams, and the results are based on weekly observations extending over a period of four years. As an indication of the varying degree of contamination to which the waters are submitted, it may be stated that Group No. I includes only waters which are collected from almost uninhabited watersheds, while Group No. IV includes waters too much polluted to be safely used for public water supplies, while the intermediate groups occupy relative positions between these two extremes. It will be observed from the diagram that the per cent. of samples which had odors of decomposition, the average amounts of chlorine, free ammonia, nitrites, nitrates and the number of bacteria per c. c. show a general increase from left to right. The nitrogens are somewhat influenced by the presence of coloring matter and microscopic organisms, so that the sequence is not as regular as might be otherwise the case. It will be observed that the per cent. of samples which gave typical positive tests for B. coli show an increase from the unpolluted waters to the polluted waters, which corresponds well with the increase of bacteria, chlorine, and odors of decomposition, and with the known facts as to the pollution of the streams in question. None of the water supplies are polluted by the discharge of sewage which directly enters the stream; in all cases the pollution may be considered as indirect. Comparisons of the results of positive tests with the rainfall shows that a majority of the positive tests have occurred immediately after heavy rains, indicating that the pollution was washed into the streams from the surface—as is known to be the case. Moreover, studies of the seasonal distribution show that there is a tendency to increased numbers of positive tests in the spring and fall, when farmers are manuring their fields. Whether or not, therefore, this test indicates absolutely the presence of B. coli in the water, it is certainly an indication of an inferior sanitary condition. The tests which have been made, using different quantities of water for each sample, have not covered a sufficiently long period for one to judge accurately as to the number of B. coli which may be allowed in a sample of water without its being considered suspicious, or unsafe to use. It has been observed, however, that almost all surface waters, even though apparently unpolluted, will give typical tests for B. coli if tested in large enough quantities; that many surface waters that would be passed as safe on the basis of ordinary chemical tests, regularly give typical

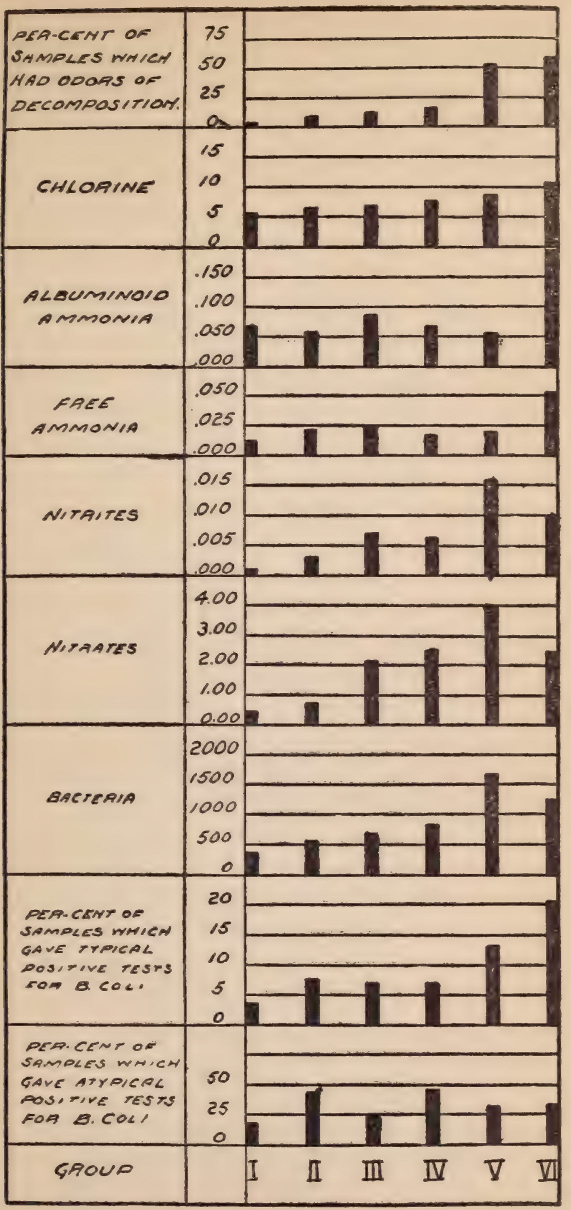


FIG. 1. — DIAGRAM SHOWING THE RELATION OF TYPICAL AND ATYPICAL PRESUMPTIVE TESTS FOR B. COLI TO THE SANITARY ANALYSES OF VARIOUS SURFACE WATERS, ARRANGED IN GROUPS ACCORDING TO THEIR RELATIVE POLLUTION.

positive tests when 500 c. c. are used; that slightly polluted waters often give positive tests with 10 c. c., while waters which are considerably polluted almost invariably give positive tests with 10 c. c. The results indicate conclusively that this modification of the test is destined to be of the very highest practical value when observations have become numerous enough to enable us to properly interpret the results.

When a typical positive presumptive test is obtained for the presence of B. coli in ground water it is an almost infallible indication of the presence of contamination with surface water. The following may be taken as an illustration:

A certain driven well station was located in swampy land, along the shores of a stream, and the tops of the wells were so placed that they were occasionally flooded at times of high water. The water in the stream was objectionable from the sanitary standpoint. The wells themselves were more than 100 feet deep, they penetrated a clay bed and yielded what may be termed artesian water. Tests for the presence of B. coli had invariably given negative results, as might be naturally expected. Suddenly, however, the tests became positive, and so continued for several days. On investigation it was found that some of the wells had been taken up to be cleaned and that the workmen in resinking them had used water of the brook for washing them down. This allowed some of the brook water to enter the system. It was also found that at the same time the water in the brook had been high, and because of the lack of packing in certain joints at the top of the wells, the brook water leaked into the suction main. The remedy was obvious, and was immediately applied, after which the tests for B. coli once more became negative. During all this time the chemical analysis of the water showed nothing abnormal in its quality. On another occasion a water supply taken from a small pond fed by springs, and which was practically a large open well, began to give positive tests for B. coli, and on examination it was found that a gate which kept out the water of a brook which had been formerly connected with the pond was open at the bottom, although it was supposed to have been shut, thus admitting a contaminated surface water to the supply.

The methods which are in vogue in England for determining the presence of B. coli in water cannot be considered as strict as those ordinarily employed in this country, and their results can be better compared with those of our presumptive tests than with the ultimate tests as made according to American methods. In the Thirtieth Annual Report of the Local Government Board of Great Britain there

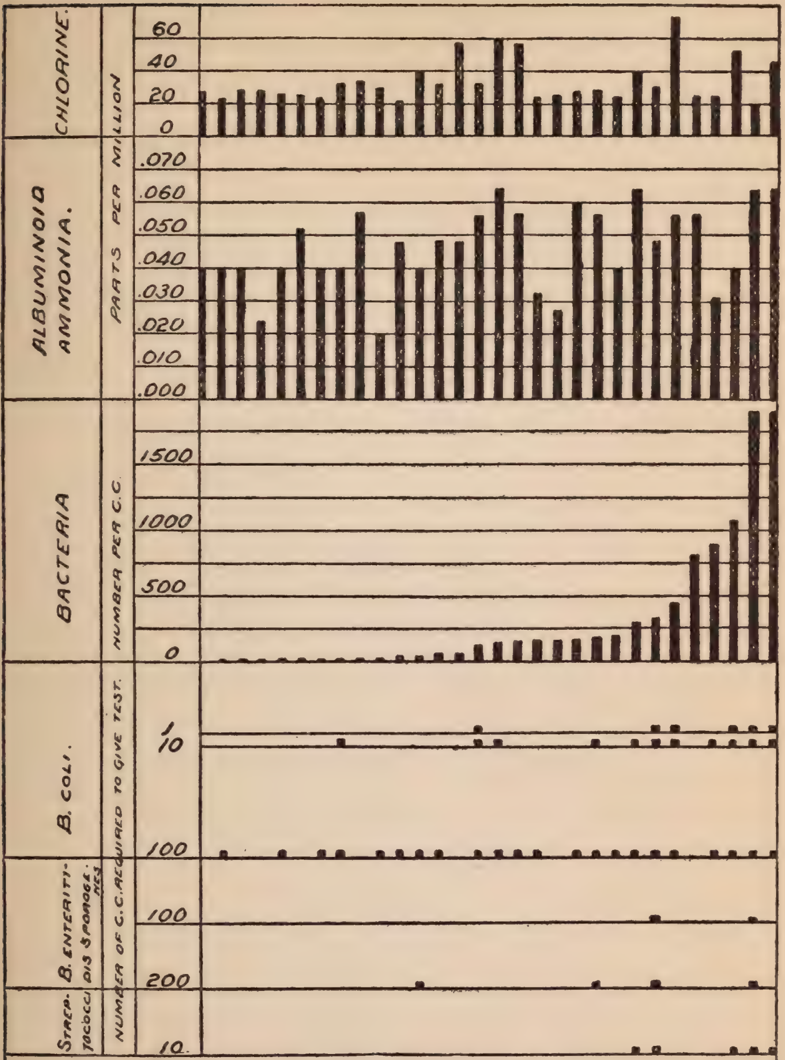


DIAGRAM SHOWING THE RESULTS OF CHEMICAL AND BACTERIOLOGICAL EXAMINATIONS OF THIRTY WELL WATERS FROM CHICHESTER, ENGLAND:

TABULATED FROM REPORT OF DR. A.C. HOUSTON.
AND ARRANGED IN ORDER OF INCREASING
NUMBERS OF BACTERIA PER C.C.

G.C.Whipple
SANITARY BIOLOGIST

FIG. 2.

is an interesting report by Dr. A. C. Houston on "The Chemical and Bacteriological Examination of Chichester Well Waters." He gives a series of figures showing the results of chemical and bacteriological tests of a large number of open wells. These results are shown graphically in Figure 2, arranged in order of increasing number of bacteria. It will be seen from this table that there was little correspondence between the total number of bacteria and either the chlorine or the albuminoid ammonia, while the results obtained by the bacteriological examination seemed to hold together very satisfactorily. It will be noticed that there was a gradual increase in the number of B. coli in the water from the left to the right of the diagram, thus corresponding with the increase in the total number of bacteria. The test for Bacillus enteritidis sporogenes and streptococci gave corresponding results.

The practical value of the presumptive test is also shown when used to determine the efficiency of filters. This has been well illustrated by the experiments made at Lawrence, Mass., by the State Board of Health, the results of which have been already presented to this Section. The following example taken from the records of a test of a mechanical filter is also of interest in this connection:

For fifteen days, during which the filter was doing good work, as shown by the chemical analysis and by its removal of bacteria, 6.7 per cent. of the samples of raw water gave positive tests when tested with 1 c. c., while the per cent. of samples of filtered water which gave positive tests was 0. When tested with quantities of 500 c. c., 40 per cent. of the samples of raw water gave positive tests and 6.7 per cent. of the samples of filtered water. During a period of sixteen days, when the filter was doing poor work, as shown by the chemical analysis and general appearance of the effluent, 18.7 per cent. of the samples of raw water gave positive tests in 1 c. c., while 31.2 per cent. of the samples of filtered water gave positive tests. When tested in quantities of 500 c. c. 56.2 per cent. of the samples of raw water gave positive tests and 56.2 per cent. of the samples of filtered water gave positive tests. Thus, during the first period the filter was yielding a water which contained very few B. coli, as determined by the presumptive tests, while during the second period the filtered water was no better than the raw water.

In conclusion, I do not wish to be understood as placing greater value upon the method used at Mount Prospect Laboratory for obtaining presumptive tests for the presence of B. coli than upon those methods in which the germ is actually isolated and studied in subcultures; several of these more elaborate methods are also used in

Mount Prospect Laboratory as occasion requires. I claim only that the presumptive test is a practical working test and very serviceable in the supervision of water supplies where the time element is an important factor; and when applied to different quantities of a sample of water it furnishes a good index of its sanitary condition.

EXPERIMENTS WITH THE NEUTRAL RED TEST FOR BACILLUS COLI IN THE EXAMINATION OF WATER SUPPLIES.

By FREDERICK S. HOLLIS, New Haven, Conn.

The method heretofore used in this laboratory for the detection of bacillus coli consisted of the tests for gas production and liquefaction of gelatin, when grown in dextrose gelatin; for indol, when grown in Dunham solution; for the coagulation of milk and the formation of acid and the determination of the per cent. of gas produced in the fermentation tube from 1 per cent. dextrose bouillon. These were preceded by preliminary cultivation in carbol bouillon and the isolation of the colonies on the lactose-litmus agar plate. Frequently, although not always, the gas ratio was determined.

In January, 1902, the neutral red agar test was included on account of the general interest felt by bacteriologists in the method and in the hope that it might justify the expectations expressed for it and perhaps serve as a quick, reliable test independently of the others. The material and method first employed was that described by Dr. William G. Savage (Journal of Hygiene, 1901), according to which 0.1 c. c. of a 0.5 per cent. of watery solution of Grübler's neutral red is added to each 10 c. c. of 0.5 per cent. glucose agar. The tubes were incubated at 37° C. and observations made every twelve or twenty-four hours.

It was soon found that it was desirable to plant as much as 10 c. c. of many of the waters received here for examination. If care is taken to use the melted agar when it is of just the right temperature and the water is mixed with it promptly before the agar has a chance to set, there results 15 c. c. of a thin jelly of a clear rose-pink color in which the bacillus coli seems to develop more readily than in the denser agar medium. Should the agar set before the water is uniformly mixed with it and appear as colored flakes, it is better to discard it and plant in another tube. The canary-yellow spots which result from the development of the individual colonies of bacillus coli are plainly marked against the pink of the media, and seem to be of a more clear yellow color from their first appearance than in the darker original neutral red agar.

As 1, 5 and 10 c. c. of the water are planted in neutral red agar, sterilized distilled water is added in such an amount to those tubes containing less than 10 c. c. of water as to serve to bring the volume to 15 c. c. As the work progressed it was observed that the development was somewhat slow when the media was thus diluted with twice its volume of water, and it was supposed that it might be due to the decrease in the food value of the mixture. This was remedied during the progress of the tests by using agar culture media containing the usual amount of agar and neutral red, but of three times the food value in peptone and meat extract; and a considerable proportion of the results reported were obtained from the use of this richer media.

It was hoped that the neutral red test would be of most value in giving an indication of the presence of bacillus coli without the necessity of preliminary cultivation, and a restrainer was used to retard the growth of other forms. Additional tubes containing no restrainer were inoculated with the same amount of water to serve as checks on the action of the restrainer.

Parietti solution was at first used, but the hydrochloric acid seemed to act deleteriously, and certain apparent irregularities in the early part of the work are believed to be due to its presence. A phenol solution was made of such strength that 1 c. c. added to the 15 c. c. of combined neutral red agar and water amounted to adding .1 per cent. of phenol and a series of tests undertaken at different seasons of the year to determine the amount that could safely be added as a restrainer. Bacillus coli that has been developed by proper preliminary cultivation and, in warm weather, a sewage effluent high in this form will withstand the presence of .15 per cent. of carbolic acid, but generally fail to develop or at least to reduce the neutral red if .2 per cent. is present. The development of bacillus coli as found in most water samples is generally retarded too much if not checked by .1 per cent. of carbolic acid, especially during cold weather, on account of which it may be advisable to vary the amount of restrainer with the season. An amount equal to .05 per cent. of carbolic acid was chosen, as that amount acted favorably in keeping down other forms, while it clearly influenced the development of the bacillus coli somewhat in the case of a surface water, as is shown by many such series as the following, while a much larger amount does not interfere with the reduction in the case of a sewage effluent:

REDUCTION OF COLOR IN NEUTRAL RED AGAR TUBES.

SURFACE WATER, JUNE 3, 1902.

	2 days.	4 days.	7 days.
1 cc water + .05% carbolic acid....	Negative	Negative	Negative
5 " + .05% " "	Negative	Partial	Partial
10 " + .05% " "	Negative	Partial	Partial
10 " + .0% " "	Complete	Complete	Complete

SEWAGE EFFLUENT, JULY 12, 1902.

			2 days.	4 days.	7 days.
1 cc effluent	+ .05%	carbolic acid	Complete	Complete	Complete
10 “	+ .05%	“ “	Complete	Complete	Complete
10 “	±1.0 %	“ “	Partial	Complete	Complete
10 “	+1.0 %	“ “	Slight	Partial	Nearly complete

The same amount of restrainer, .05 per cent. carbolic acid, was also used with the 100 c. c. of water that was grown in bouillon for three days at 37° C. from which the regular tests for bacillus coli were to be made. Having grown the sample of water for three days in .05 per cent. carbol bouillon and having also established the neutral red test, lactose-litmus agar plates were made from the carbol bouillon and from the neutral red tube showing the clearest reduction of the three to six that were generally grown; and from these the bacillus coli was generally, although not always, passed through one or more tubes of bouillon before inoculating the media for the distinctive tests.

In cases where these media were inoculated directly from the acid forming colonies on the lactose-litmus agar plate, it was almost always found that the growth in the carbol bouillon or the dilute and highly nutrient neutral red agar had served well for the development of the form.

In the following tables are grouped, according to their source, the analyses of all the samples that have been submitted from January to July, 1902, and it will be seen that, with the exception of the few ice samples and part of the well waters, practically all gave positive growths in .05 per cent. carbol bouillon and positive neutral red tests in at least one of the three to six neutral red tubes containing varying amounts of restrainer and varying amounts of water; and that, with few exceptions, these in turn gave acid colonies on lactose-litmus agar plates and the other characteristic tests for bacillus coli, as well as did the sets inoculated from the carbol bouillon flasks.

The almost perfect parallelism of results between the final tests for bacillus coli from the carbol bouillon growth and the positive neutral red agar tubes indicates that an opinion as to the presence of this form from the positive neutral red tests would, in most of the cases studied, have been the same as one based on the usual tests.

In most cases wherever there are irregularities in the final tests made from the carbol bouillon there are corresponding irregularities in the final tests made from the neutral red agar tube culture, and in some cases there are positive neutral red tests that are not verified in the final tests, even though it showed a positive reduction. This would lead one to accept with caution a positive neutral red test as conclusive evidence of the presence of bacillus coli.

It is evident that the method must be put to the test in every possible way before agreement can be reached as to its complete value; but the results of this series, as well as other series already before bacteriologists, seem to warrant the belief that the method has a value when considered with the other tests, and that a negative result in the case of a neutral red test would undoubtedly indicate the absence of bacillus coli.

The fermentation tests, while made with care and generally under the same conditions, show marked irregularities both as to the amount of gas and the gas ratio.

Work which I have undertaken here and which I hope to be able to continue seems to indicate that the gas ratio especially depends directly upon the period of growth when taken and that in order to obtain a ratio of $\frac{H}{CO_2} = \frac{2}{1}$, the accepted one for B. coli, it must be taken after a short period of growth.

The author wishes to acknowledge his indebtedness to Dr. Herbert E. Smith, chemist of the Connecticut State Board of Health, and Professor of Chemistry of the Yale Medical School, for valuable suggestions and for his kind interest in the work during its progress.

Surface Waters - Reservoirs and Ponds

1 - Inoculated from Lactose Bouillon
2 - Inoculated from Neutral Red Agar.

Source	Growth in Lactose Bouillon	Reduction of Neutral Red		Gas	Liquefaction	Indol	milk Coagulation	Acid formation	Fermentation Test % Gas	H	Ratio	Bacteria per c.c. Gel. Plate
Woodbridge Jan 31'02	neg	Pos	1	—	—	—	—	—	—			619
			2	Pos	neg	neg	Pos	Pos	80			
New Haven Jan 31'02	Pos	Pos	1	neg	neg	neg	Pos	Pos	12			1410
			2	Pos	neg	neg	Pos	Pos	46			
Greenwich July 28'02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	56	22	22/34	2220
			2	Pos	neg	Pos	Pos	Pos	100	44	44/66	290
Greenwich July 28'02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	20	18	18/2	299
			2	Pos	neg	Pos	Pos	Pos	56	18	18/38	
Woodbridge Feb 18'02	Pos	Pos	1	neg	neg	neg	neg	neg	neg			53
			2	neg	neg	neg	Pos	Pos	60			
New Haven Feb 18'02	Pos	Pos	1	neg	neg	neg	Pos	Pos	neg			297
			2	neg	neg	neg	Pos	Pos	100			
Woodbridge Mar 5'02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	65	54	54/31	7570
			2	Pos	neg	Pos (al)	Pos	Pos	86	30	30/56	
Orange Mar 5'02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	40	22	22/18	4560
			2	Pos	neg	Pos	Pos	Pos	33	25	25/8	
East Haven Mar 5'02	Pos	Pos	1	neg	neg	neg	Pos	Pos	neg			1375
			2	neg	neg	neg	Pos	Pos	49	37	37/12	
New Haven Mar 5'02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	39	22	22/17	6077
			2	Pos	neg	Pos (al)	Pos	Pos	67	49	49/18	
Hamden Mar 5'02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	29	19	19/10	835
			2	neg	neg	neg	Pos	Pos	neg	neg		
Stamford Mar 7'02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	95			906
			2	Pos	neg	Pos	Pos	Pos	90			
Woodbridge Apr 2'02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	37			275
			2	neg	neg	neg	neg	neg	—			
New Haven Apr 2'02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	33			980
			2	Pos	neg	Pos (al)	Pos	Pos	58			
Woodbridge May 2'02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	28			309
			2	—	—	—	—	—	30	20	20/10	
New Haven May 2'02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	58			314
			2	—	—	—	—	—	40	25	25/15	
Woodbridge June 3'02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	70	29	29/41	420
			2	Pos	neg	Pos	Pos	Pos	49	19	19/30	
Orange June 3'02	Pos	Pos	1	neg	neg	Pos	Pos	Pos	70	31	31/39	370
			2	Pos	neg	neg	Pos	Pos	19	15	15/4	
East Haven June 3'02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	30	22	22/8	140
			2	Pos	neg	neg	Pos	Pos	7	7	—	
New Haven June 3'02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	25	18	18/7	130
			2	Pos	neg	Pos	Pos	Pos	60	26	26/34	
Hamden June 3'02	Pos	Pos	1	Pos	neg	Pos	Pos (al)	Pos	41	18	18/23	350
			2	neg	neg	Pos	Pos	Pos	49	19	19/30	
Bridgeport June 9'02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	39	17	17/22	445
			2	Pos	neg	Pos	Pos	Pos	72	30	30/42	
Bridgeport June 9'02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	41	19	19/22	205
			2	Pos	neg	Pos	Pos (al)	Pos (al)	48	25	25/23	
Bridgeport June 9'02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	50	22	22/28	370
			2	neg	neg	Pos	Pos	Pos	90	38	38/52	
Bridgeport June 9'02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	0	0		65
			2	Pos	neg	Pos	Pos	Pos	—	—		
Bridgeport June 9'02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	50	23	23/27	265
			2	neg	neg	Pos	Pos	Pos	—	—		
Woodbridge July 1'02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	50	22	22/28	200
			2	Pos	neg	Pos	Pos	Pos	47	22	22/25	
Orange July 1'02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	59	25	25/34	745
			2	Pos	neg	neg	Pos	Pos	0	0		
East Haven July 1'02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	30	21	21/9	70
			2	Pos	neg	Pos	Pos	Pos	49	26	26/23	
New Haven July 1'02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	57	26	26/31	60
			2	Pos	neg	Pos	Pos	Pos	34	22	22/12	
Hamden July 1'02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	50	21	21/29	115
			2	Pos	neg	Pos	Pos	Pos	51	23	23/28	

Surface Waters – Rivers and Streams

1 Inoculated from Carbol Bouillon
2 Inoculated from Neutral Red Agar

Source	Growth in Carbol Bouillon	Reduction of Neutral Red		Gas	Liquefaction	Indol	Milk Coagulation	Milk acid formation	Fermentation Rate % Gas	H	Ratio	Bacteria per cc Gel Plate
Connecticut River Windsor Locks July 15 '02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	52	25	25/27	877
			2	Pos	neg	Pos	Pos	Pos	39	18	18/21	
Connecticut River Hartford July 25 '02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	54	31	31/23	1690
			2	Pos	neg	Pos	Pos	Pos	38	21	21/17	
Housatonic River July 24 '02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	62	29	29/33	
			2	Pos	neg	Pos	Pos	Pos	53	30	30/23	803

Ice Samples.

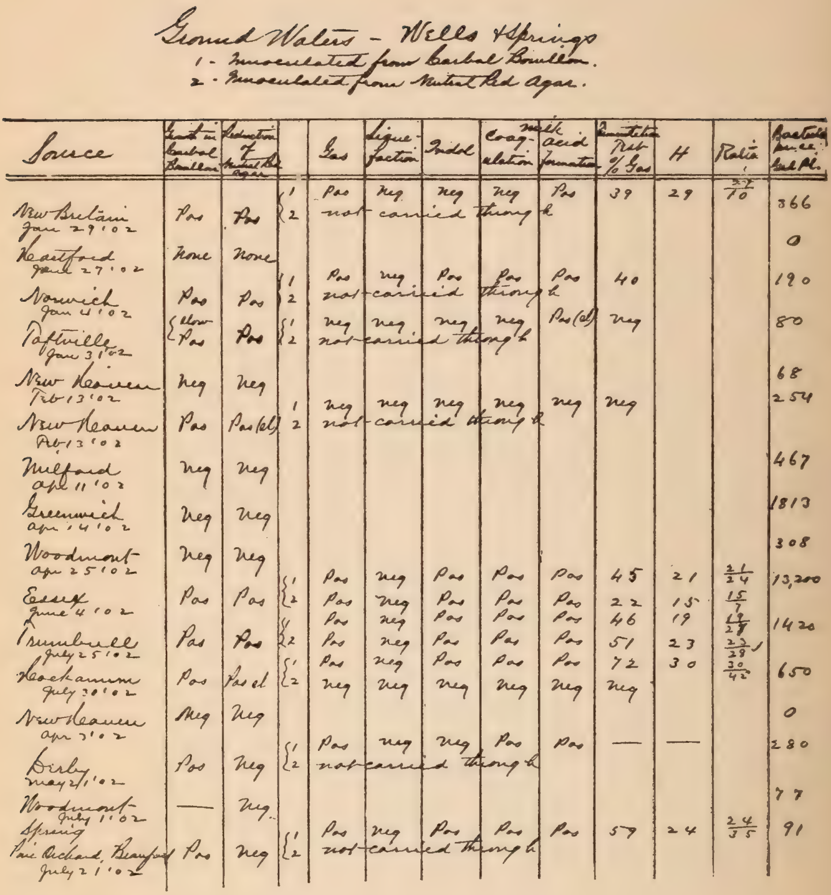
1 Inoculated from Carbol Bouillon
2 Inoculated from Neutral Red Agar.

Source	Growth in Carbol Bouillon	Reduction of Neutral Red		Gas	Liquefaction	Indol	Milk Coagulation	Milk acid formation	Fermentation Rate % Gas	H	Ratio	Bacteria per cc Gel Plate
Naugatuck Mar 19 '02	Pos	neg	1	neg	—	—	—	—	neg			35
			2	—	—	—	—	—	—			
Naugatuck Apr 10 '02	Pos	neg	1	neg	—	—	—	—	neg			4
			2	—	—	—	—	—	—			
Hartford May 6 '02	Pos	neg	1	neg	neg	neg	Pos	Pos	48	25	25/23	1
			2	—	—	—	—	—	—			

Sewage Effluents

1 Inoculated from Carbol Bouillon
2 Inoculated from Neutral Red Agar.

Source	Growth in Carbol Bouillon	Reduction of Neutral Red		Gas	Liquefaction	Indol	Milk Coagulation	Milk acid formation	Fermentation Rate % Gas	H	Ratio	Bacteria per cc Gel Plate
Norfolk July 29 '02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	22	16	16/6	2700
			2	Pos	neg	Pos	Pos	Pos	58	24	24/34	
Hamden July 12 '02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	38	14	14/24	
			2	Pos	neg	Pos	Pos	Pos	40	15	15/25	2970



Ground Waters – Wells & Springs

1 – Inoculated from Carbol Bouillon.

2 – Inoculated from Neutral Red Agar.

Source	Growth in Carbol Bouillon	Reduction of Neutral Red Agar		Gas	Lique-faction	Indol	milk Coag-ulation	acid formation	Fermentation Tube % Gas	H	Ratio	Bacteria per cc. Gel. Pl.
New Britain Jan 29 '02	Pos	Pos	1	Pos	neg	neg	neg	Pos	39	29	29/10	366
			2	not carried through								
Hartford Jan 27 '02	None	None										0
Norwich Jan 4 '02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	40			190
			2	not carried through								
Taftville Jan 31 '02	slow Pos	Pos	1	neg	neg	neg	neg	Pos (sl)	neg			80
			2	not carried through								
New Haven Feb 13 '02	neg	neg										68
New Haven Feb 13 '02	Pos	Pos (sl)	1	neg	neg	neg	neg	neg	neg			254
			2	not carried through								
Milford Apr 11 '02	neg	neg										467
Greenwich Apr 14 '02	Neg	Neg										1813
Woodmont Apr 25 '02	Neg	Neg										308
Essex June 4 '02	Pos	Pos	1	Pos	neg	Pos	Pos	Pos	45	21	21/24	13,200
			2	Pos	neg	Pos	Pos	Pos	22	15	15/7	
Trumbull July 25 '02	Pas	Pos	1	Pos	neg	Pos	Pos	Pos	46	19	19/27	1420
			2	Pos	neg	Pos	Pos	Pos	51	23	23/28	
Hockanum July 30 '02	Pos	Pos sl	1	Pos	neg	Pos	Pos	Pos	72	30	30/42	650
			2	neg	neg	neg	neg	neg	neg			
New Haven Apr 3 '02	Neg	Neg										0
Derby May 21 '02	Pos	Neg	1	Pos	neg	neg	Pos	Pos	—	—		280
			2	not carried through								
Woodmont July 1 '02 Spring	—	Neg										77
Pine Orchard, Branford July 21 '02	Pos	neg	1	Pos	neg	Pos	Pos	Pos	57	24	24/35	91
			2	not carried through								

THE USE OF IMMUNE SERUM IN THE SEPARATION OF TYPHOID FEVER FROM COLON BACILLI.

By ADOLPH GEHRMANN, M. D., Chicago.

The use of coli immune serum for the purpose of assisting the isolation of typhoid bacilli in the presence of colon bacilli has been reported by L. Biffi[1] and by A. Moore.[2] Biffi tried isolation by means of a Berkfeld filter in the hope that the more active motility of typhoid would bring about a penetration of the filter medium by growing through before the less active colon bacilli. This was found unsatisfactory. Later he proposed to first agglutinate the colon bacilli with colon immune serum in the hope that the clumps thus formed would further prevent its growing through. The value of the method is thus far without confirmation.

Moore proceeds by using a piece of glass tubing bent into W shape and filled with bouillon or gelatin. In one arm the suspected material plus a varying quantity, several drops, of coli immune serum, is placed. In the incubator the colon bacilli are agglutinated while the active motile typhoid bacilli grow through the semi-liquid gelatin and finally appear in the farther arm in pure culture. He noted the length of time required for the typhoid bacilli to reach the opposite side and found that it was twenty to thirty hours. Experimentally with pure cultures the results were encouraging, but the practical tests do not lead Moore to recommend the method very highly.

It would seem that the first point to be considered in these procedures is as regards the immunization of animals against colon bacilli and the action of the immune serum produced.

The investigation of serum reaction with B. coli has been investigated by Pfoundler, Wolf, Rodet and Deelemann, while more recently Rothberger[3] has added considerable to this subject. His experiments on 37 strains of coli bacilli would seem to show conclusively that there is a wide variation in the results. When an animal is immunized with one strain there is no regularity in regard to the reaction of this serum towards the other strains. In only a few instances did the others give positive agglutination with the serum, but in all except two instances the culture used in immunization promptly agglutinated. Combined immunization with several strains gave as irregular results. A close study of the protocol of his work will give one a decided impression that it is practically impossible to produce a serum that is capable of causing a positive reaction with the colon bacillus in the general acceptation of the group of organisms included under this name.

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[1] L. Biffi. Su di un nuovo d' isolamento del bacillo del tifo. Riforma Medico, Jan., 1902.

[2] A. Moore. Isolation of the typhoid bacillus. Brit. Med. Jour., March, 1902, p. 703.

[3] J. Rothberger. Agglutination f. B. Coli. Zuto f. Hyg. u infecto. Bd. xxxiv, Heft 1, p. 79.

If these results stand and it is impossible to obtain a suitable serum then certainly there can be no value to the methods. If an immune serum active for one or the other species can be used to restrain the growth of one, without disturbing the growth of the other, it would be an ideal method.

All of the many methods using special media for the cultivation of *Bacillus typhosus* involve the same principle in that colon typhoid group is separated from the other organism and all leave the exact differentiation as before a matter of further tests that involve considerable difficulty. The colon bacillus always stands in the way of typhoid isolation and by an active overgrowth and comes to be the organism that is in reality isolated. If the usual result could be reversed and an overgrowth by typhoid established, we would no doubt become acquainted with some interesting facts. The direct precipitation of bacteria that remain suspended in a liquid medium by the use of immune serum is a possibility and a method that will very quickly reduce the number or entirely remove them at once appeals to one as rational. It is to be compared with the chemist's methods of precipitating one salt after another from solutions. As far as the typhoid bacillus is concerned, we cannot hope for an accurate study of the distribution of this organism, until our culture methods will give a rapid and positive overgrowth when it is present, and make its isolation certain and easy.

In conducting these experiments the following lines of investigation were followed:

1. Immunization of rabbits.
2. Agglutinating power of blood for B. coli.
3. Agglutinating power for B. typhoid.
4. Action on these bacteria in mixtures.
5. Action on mixtures in presence of carbolic acid.
6. Practical use of fluid method.
7. Practical use of method using filter paper after manner of Berkfeld filter.

IMMUNIZATION.

For the purpose of testing the method three rabbits were injected with colon cultures, beginning September 2d, last, and continuing to date. On one, a colon culture isolated from the blood of a typhoid patient was used, and on the other two, colon cultures from water. One of these latter cultures (culture 3) was selected because it showed considerable motility in bouillon. The rabbits always received small doses, .2 to .5 c. c. of a 24-hour bouillon growth subcutaneously injected. The injections were not made at regular intervals, but depended largely upon the production of swelling and its disappearance. The rabbits were full grown and did not lose in weight or appear to be injured by the injections. After October 1 blood was repeatedly obtained by bleeding from the ear in the usual manner. The production of a mixed serum was not attempted. The blood was directly diluted with bouillon or dissolved after it had been dried.

Reaction with colon bacilli. The blood of all three rabbits shows positive reaction with the individual culture used for immunization. The blood of rabbit No. 2 (non-motile culture from water) was positive to the colon from man No 1 and negative to the motile water colon bacillus No. 3. Numbers 1 and 3 were positive for the other two as well as for the culture used in their preparation. No. 3 rabbit gave the most positive results.

The reaction toward typhoid. The typhoid culture used in testing the bloods from the three rabbits was that regularly used for Widals. All the bloods were tried upon several occasions against this culture. It was found that No. 3 rabbit (motile water bacillus) reacted to some extent on the typhoid. In dilutions 1:50 to 1:10 there was well marked clumping, but never a complete loss of motility. The other bloods, No. 1 and No. 2, gave no reaction with typhoid.

Action on mixture of the colon bacilli and typhoid. Tubes containing 5 and 10 c. c. of bouillon were prepared by adding a varying number of drops of the rabbit's blood directly from the ear and then planting them with colon and typhoid from 24-hour bouillon cultures. The dilutions were 1:100 and 1:200 for the blood. All of the tubes were placed in the incubator for 4 to 6 hours, at the end of which hanging drops and transplantations to slant agar were made. It was soon apparent that the examination and transfers should be made within a few hours after preparing the mixtures because of the tendency of the colon bacillus to overgrow, if a longer time was given. This is no doubt the cause of failure in the two methods above described.

All of the dilutions of 1:200 serum showed the typhoid bacilli undisturbed in motility and the colon bacilli more or less well agglutinated. In the dilutions 1:100 the results were very irregular, so that finally the experiments were made on tubes containing serum

1:200. From the mixtures there was no great difficulty in isolating the typhoid bacilli by means of colonies obtained on slant agar. The transfer material was always taken from the surface of the bouillon in the tubes. Mixture of typhoid and colon bacilli that had been placed in the incubator for 24 to 48 hours gave very irregular results and in only a few instances was it possible to recover the typhoid.

The use of carbolized broth. Following the same lines as those in the experiments using bouillon, broth containing .1 per cent. carbolic acid was used. The broth contained no glucose. The same general results were obtained showing that the presence of this amount of carbolic acid did not interfere with the results. Further, it was found that when a dilution of fæces was added, the restraining effect of the carbolic acid prevented an overgrowth of saprophytes.

THE PRACTICAL USE OF THE METHOD.

As to the general results that we have had it must be said that they are not conclusive. The greatest difficulty is in a control and this involves the most widely diverse conditions to be artificially produced and studied. What is certain thus far is that the two species in pure culture can be separated by this means.

Moore reports inability to isolate B. typhoid from the fæces of typhoid cases, because, as he believes, the serum prepared against a single strain of B. coli does not act on other varieties present and an overgrowth is the consequence.

The W shaped tubes of Moore offer no special advantages, because of the length of time required to grow through the medium and the danger of overgrowth.

The use of gelatin as against bouillon shows an advantage for the former because the bacteria settle more rapidly in it and the motile group is less disturbed in its activity.

The method of using filter paper. Opportunity was not taken to test the scheme prepared by Biffi, in which the Berkfeld filter is used. However, a modification was attempted. This consisted in using the cartridges of paper such as are used in fat extraction methods. This paper offered the possibility of confining the agglutinated bacilli and not as much resistance to the typhoid bacillus. The procedure consisted in placing the immune blood and mixture of bacteria within the cone of paper and testing the bouillon outside by cultures and hanging drops for the presence of typhoid bacilli. The results both with the pure cultures and with ordinary material, water, fæces, etc., was unsatisfactory chiefly because of the length of time required for the bacteria to get through the paper.

The conclusions that present themselves are:

First—That immune colon serum gives irregular results in agglutination of members of the colon group.

Second—That the separation of pure cultures of the two species is facilitated by this method.

Third—Practically, results of little value are shown.

LABORATORY METHODS USED DURING THE NEW ORLEANS WATER PURIFICATION INVESTIGATION.

By ROBERT SPURR WESTON, Boston, Mass.

Perhaps it would be interesting to some of the members of this Section if a partial description of the methods of chemical and bacteriological analyses used during the water purification at New Orleans should be given. During this investigation several thousand analyses were made and the methods laid down in the report of the Committee on Methods of Water Analysis were departed from in some particulars, as follows:

SILICA TURBIDITY.

At the beginning of the investigation it was determined to express all the results of turbidity determinations in terms of equivalent amounts of suspended silica (diatomaceous earth), as proposed by Whipple and Jackson[1] in 1899; that is, the turbidity of the water was equal to the number of parts of silica per million parts of water required to produce the same degree of turbidity. When the samples of water had a turbidity of less than 40, they were compared directly with standard suspensions of silica, both contained in white glass bottles. But because of the inaccuracies which arose when more turbid samples were compared directly, it was convenient to make use of a diaphanometer, which was calibrated in terms of the standard silica suspension.

The diaphanometer used was similar to that used by Parmelee and Ellms at Cincinnati, and described elsewhere.[2] It consisted of a tube about 1 meter (40 inches) long and 30 centimeters (1.2 inches) in diameter. This tube was supported upon a box, in which were placed the lighting arrangements. At the lower end of this tube there was a disk of white glass, four quadrants of which were partially blackened, thereby leaving a transparent cross with arms one millimeter wide. This "cross-of-light" corresponds to the wire of the wire method, and the turbidity is determined by noting the vanishing point of this cross when the same is viewed against a strong light.

This cross was lighted from below by means of a Welsbach light, the rays of which first passed through a double plano-convex condenser and were reflected up through the tube from an inclined convex mirror. The condenser and convex mirror helped to destroy the image of the Welsbach mantel.

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[1] Technology Quarterly, Boston, Mass., Vol. XII, No. 4, December, 1899.

[2] Parmelee & Ellms, Technology Quarterly, Vol. XII, 1899, p. 145.

The instrument was used as follows: After the burner was lighted, water was poured in at the point near the top of the tube until the "cross-of-light" vanished from view.

The diaphanometer was standardized in a manner analogous to that employed by Hazen and Whipple[1] in standardizing the wire instrument with which you are indeed familiar. All results were expressed in terms of standard silica suspension, as determined from experiments made with the diaphanometer, to determine the depths at which standard suspensions of silica would cause the "cross-of-light" to vanish. Thus, a water which had a turbidity of 100 caused the "cross-of-light" to vanish at the same point as a suspension containing 100 milligrams of silica to the liter.

For the purpose of comparison, turbidities were also determined by means of the wire method. This was done on a large number of samples, in order to determine the comparative values of the two methods.

In general the experiences at New Orleans went to show that determinations could be made more precisely with the diaphanometer than with the wire instrument, but that the latter was much more convenient. Furthermore, determinations could be made much more rapidly with the wire instrument.

The diaphanometer is especially useful in laboratories connected with certain filter plants, where it is necessary to vary the amounts of applied coagulant according to the varying turbidity of the applied water. As it is necessary to vary the same during the night time, the diaphanometer is very useful because it is independent of daylight, while the wire method may be used only during the hours of daylight, and is best used when instantaneous pictures could be taken with a camera. The wire instrument seems to be the best for general field work, especially the instrument recently devised for the U. S. Geological Survey.

At New Orleans it was found that single determinations made by the diaphanometer varied from time to time with different intensities of light, different observers and different suspensions of silica. The total variation reached fifteen per cent. at times; the personal error of reading was as high as seven per cent. among three or four observers.

The following table shows the relation between the various methods for determination of turbidity, as practiced at New Orleans. One column gives the equivalent values of the silica scale of the U. S. Geological Survey, as described in a recent circular.[1]

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[1] Jour. New Eng. W. W. Assn., Vol. XVI, No. 4, 1902. Also Circular No. 8, U. S. Geol. Survey, Division of Hydrography, 1902.

TABLE SHOWING THE RELATION BETWEEN THE VARIOUS METHODS OF TURBIDITY DETERMINATION.

By direct comparison with silica standards.	By diaphanometer expressed as parts of silica per million.	Silica scale of U. S. Geol. Survey.[1]	Wire reciprocal of vanishing depth in inches.
5			0.018
10		12	0.037
15		16	0.049
20		24	0.070
25		27	0.078
30		32	0.090
40	40	45	0.12
50	50	55	0.15
60	60	68	0.18
	70	81	0.21
	80	94	0.24
	90	109	0.27
	100	121	0.30
	120	150	0.35
	140	175	0.40
	160	200	0.44
	180	225	0.48
	200	250	0.52
	220	275	0.55
	240	295	0.58
	260	325	0.62
	280	355	0.66
	300	380	0.69
	320	400	0.72
	340	440	0.76
	360	470	0.79
	380	500	0.82
	400	540	0.86
	450	620	0.94
	500	720	1.02

The above are average determinations on samples from all sources, Mississippi River, and settling basin effluents.

It will be seen that there are some differences between the turbidity scale of the U. S. Geological Survey and that established at New Orleans, or, as Hazen expresses it, the values of the standard vanishing depth expressed in terms of silica, differ. It was impracticable to exactly determine the reasons for this difference while the work was in progress. Possibly the differences may be charged to differences in the silica used as a standard of comparison.

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[1] Hazen and Whipple. Circular No. 8, U. S. Geological Survey, Division of Hydrography, 1902.

Many of the determinations were checked by gravimetrical determinations of suspended matter. When these were compared with the turbidity determination, it was soon observed that when the Mississippi River partook largely of the character of its southwestern tributaries (the Red and Arkansas rivers), a given amount of suspended matter would produce more turbidity from an optical standpoint than the same amount of suspended matter when the river water partook largely of the character of some of its other tributaries—notably the Upper Mississippi. This phenomenon led to the adoption of the expression, "turbidity coefficient," which is a measure of the character of the particles producing the turbidity, and is a ratio between the suspended matter determined gravimetrically and the turbidity expressed as the equivalent number of parts of silica per million:

$$\text{Turbidity coefficient} = \frac{\text{Suspended matter.}}{\text{Turbidity.}}$$

Waters containing coarse, crystalline silt particles, which have a low turbidity-producing power, have a high turbidity coefficient, while the opposite is true of waters containing finely divided clay particles.

The following table gives the average turbidity coefficients observed at New Orleans:

TABLE OF AVERAGE TURBIDITY COEFFICIENTS.

Water.	Turbidity Coefficient.
Mississippi River water	1.08
" " " after 6 hours' subsidence	0.90
" " " " 12 " "	0.87
" " " " 18 " "	0.86
" " " " 24 " "	0.85
" " " " 48 " "	0.80
" " " " 72 " "	0.76
" " " " 24 " subsidence and coagulation	0.60

This table is very easy to understand, because the coarser particles of low turbidity-producing power and somewhat higher specific gravity, gradually separate out according to their hydraulic values, the finer particles of high turbidity-producing power and somewhat lower specific gravity remaining longest in suspension.

DETERMINATION OF SUSPENDED MATTER—GRAVIMETRIC.

The suspended matter was determined by estimating the residue on evaporation of the water, both before and after being passed through a Berkfeldt filter tube, the difference between these results being the suspended matter. Pasteur filter tubes were tried, but were found to introduce errors into the determination, while the error arising from the use of the Berkfeldt filter tube which was used throughout most of the investigation, was negligible. All residues were dried at 105° C. for at least one hour before cooling and weighing. Much difficulty was experienced during the rainy summer months in weighing the dishes containing the residues; in fact, for two weeks at one time during a period of daily showers, the humidity was so great that the correct weighing of the residues was impossible. All results of this determination are recorded to the nearest five parts per million, in order to escape fictitious accuracy.

BACTERIAL.

The methods used for the determination of species were described by Arthur I. Kendall and the speaker at the Buffalo meeting of this Association.

METHOD OF ISOLATING BACILLUS ENTERIDITIS SPOROGENES (KLEIN).

The following method was used for the isolation of *bacillus enteriditis sporogenes*. From 1 to 25 cubic centimeters of the sample of water were mixed with from 10 to 100 cubic centimeters of sterile neutral milk. The cultures were heated in a water bath to a temperature of 80° for fifteen minutes. The whole was then put into an atmosphere of hydrogen and incubated for two days at 37° C. Cultures which then produced a characteristic coagulation of the milk and an odor of butyric acid were inoculated into guinea pigs, one cubic centimeter of the clear whey being allowed to each 200 grams of animal weight. If the guinea pigs died with the formation of characteristic lesions, the identity of the bacillus was considered established.

METHOD FOR BACILLUS COLI COMMUNIS.

The method used for the isolation of *B. coli communis* varied considerably during the course of the work. At the beginning, when the bacterium was occasionally isolated, at least two portions of from 1 to 25 cubic centimeters of the sample to be tested were sown into dextrose-broth and the cultures showing positive results were plated out on lactose-litmus-agar plates. Characteristic red colonies were then selected for seeding the various conventional media. Later on, however, it was necessary to concentrate a large sample of water—300 cubic centimeters, in a centrifugal machine. The concentrate was then sown into phenol-dextrose broth contained in fermentation tubes, which were incubated at blood temperature for not over 48 hours. Cultures which then showed the possible presence of the looked-for bacteria were sown on lactose-litmus-agar plates, as described above. The results of most of the above work were negative.

TEST FOR HYDROGEN SULPHID.

One test was used which may be of interest, namely, the test for hydrogen sulphid, produced by anærobes. The bacteria under examination were sown into nutrient broth contained in test tubes. A small piece of filter paper which had been soaked in a 1 per cent. solution of plumbic acetate and subsequently dried, was attached to the cotton plug. The inoculated tubes were placed in a Novy apparatus in an atmosphere of hydrogen and incubated. If the paper blackened, it was considered evidence of the presence of bacteria which broke down sulphur compounds to produce hydrogen sulphid.

A STUDY OF THE METHODS EMPLOYED FOR DETERMINING THE AMOUNT OF OXYGEN CONSUMED FROM PERMANGANATE BY THE IMPURITIES IN WATER AND SEWAGE.

By HENRY C. FULLER, S. B., Detroit, Mich.

The methods employed for the determination of the consumed oxygen depend upon the fact that permanganate of potassium in acid solution gives up oxygen to oxidizable substances, being reduced to manganous oxide.

The majority of methods in use at the present time were described in detail in an article by Professor Weems, published in the "Engineering Record" for January and February, 1901. In the present work, there were certain other methods investigated and a resumé of the entire list will be given.

ENGLISH OFFICIAL METHOD.

Sutton Volumetric Analysis, p. 430-443.
Weems Engineering Record, 1901, p. 128.

Two determinations with 250 c. c. of the water treated with 10 c. c. dilute H_2SO_4 and 10 c. c. standard $K_2Mn_2O_8$; one determination made at the end of 15 minutes and the other at the end of 4 hours. Excess of $K_2Mn_2O_8$ destroyed by KI and liberated I titrated with standard $Na_2S_2O_3$.

KUBEL METHOD.

Weems Engineering Record, 1901, p. 128.

100 c. c. of the water heated to boiling and treated with 5 c. c. H_2SO_4 and 10 c. c. standard $K_2Mn_2O_8$; and boiled 10 minutes. The excess of $K_2Mn_2O_8$ destroyed by standard oxalic acid and the solution titrated to a pink with standard $K_2Mn_2O_8$.

The many modifications of these methods occur under two classes. Those where oxidation is done in the cold and fall under the same class as the English Official Method, and those whose oxidation is done at the boiling temperature and fall under the Kubel Method.

Under the first class comes the method of the Mersey and Irwell Commission. (Journal Society of Chem. Industry, January 31, 1898, 9. 11.)

70 c. c. sewage treated with 10 c. c. H_2SO_4 and 10 c. c. standard $K_2Mn_2O_8$ for 4 hours at the ordinary temperature. Excess $K_2Mn_2O_8$ decomposed by KI and I titrated with $Na_2S_2O_3$.

BARNES METHOD.

Journal Society of Chem. Industry, 1896, p. 82.

Used in comparison with his chromic acid process. 100 c. c. water treated with 10 c. c. H_2SO_4 and 10 c. c. $K_2Mn_2O_8$ and allowed to stand at a temperature of 70° F. for 2½ hours. Excess of $K_2Mn_2O_8$ destroyed by standard ferrous sulphate, and then titrated to color with $K_2Mn_2O_8$.

BELL'S METHOD.

Journal Society of Chem. Industry, 1898, p. 11.

J. C. Bell criticised the Mersey & Irwell Method. He treated 10 c. c. of the effluent and 50 c. c. distilled water with 10 c. c. H_2SO_4 and 10 c. c. standard $K_2Mn_2O_8$ and allowed the whole to stand for 4 hours at the ordinary temperature. At the end of this time KI added and liberated I titrated against standard $Na_2S_2O_3$.

DUPRE'S METHOD.

Analyst, Vol. X, pp. 118-120.

Introduced for working with waters rich in chlorides.

250 c. c. or 500 c. c. of the water cooled by ice, treated with 10 c. c. standard $K_2Mn_2O_8$ and 10 c. c. tri-basic phosphoric acid and allowed to stand 24 hours in a refrigerator. Excess $K_2Mn_2O_8$ decomposed by KI and titrated against $Na_2S_2O_3$.

METHOD EMPLOYED AT LAWRENCE STATION.

Lawrence, Mass.

10 c. c. sewage made up to 100 c. c. with distilled water; 5 c. c. H_2SO_4 added and boiled. 10 c. c. standard $K_2Mn_2O_8$ added and boiled 2 minutes; excess of $K_2Mn_2O_8$ destroyed by standard oxalic acid and then titrated to color with $K_2Mn_2O_8$.

MASSACHUSETTS STATE BOARD OF HEALTH METHOD.

Used in Boston Laboratories.

Same as that employed at Lawrence, except that boiling is continued for 5 minutes.

METHOD EMPLOYED AT WORCESTER SEWAGE WORKS.

Worcester, Mass.

100 c. c. effluent treated with 2 c. c. H_2SO_4 and boiled. 10 c. c. standard $K_2Mn_2O_8$ added and boiled 2 minutes. Excess $K_2Mn_2O_8$ destroyed by standard ferrous sulphate and then titrated to color with $K_2Mn_2O_8$.

METHOD RECOMMENDED BY MASON.

Water Supply, p. 406.

200 c. c. of water taken in a porcelain casserole; otherwise the method is the same as the Kubel Method.

METHOD RECOMMENDED BY THE CHEM. SECTION OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

Weems Engineering Record, 1901, p. 129.

200 c. c. of the water are treated according to the Kubel mode of procedure.

SCHULZE METHOD.

Weems Engineering Record, 1901, p. 128.

100 c. c. of water heated to boiling and 2 c. c. NaOH added to 10 c. c. standard $K_2Mn_2O_8$. Boiled 10 minutes and then cooled to 50 to 60° C. 5 c. c. H_2SO_4 added, excess of $K_2Mn_2O_8$ destroyed by standard oxalic acid and then titrated to color with $K_2Mn_2O_8$.

Brown and Fales[1] in the course of research work on this test had shown that Dupre's method at 0° for 24 hours gave results which agreed very closely with those obtained by the English Official Method for four hours. That in pond water there was a close agreement, between the four-hour English Official Method and the two-minute Lawrence Experiment Station Method; but that with sewage effluent, there was no such agreement, the Lawrence Method giving results about twice as high. That the Mersey & Irwell Methods gave lower results than the English Official Method for the same sample. That with pond waters, the Lawrence Method and Worcester Sewage Works Method agreed very closely, but with sewage effluent in nearly every case, the Worcester Method gave trifle lower results. That the 15-minute English Official Test gave results about one-half as large as did the four-hour English Official Test.

In 1900 the Committee on Standard Methods of Water Analysis of the American Public Health Association sent out to leading workers a circular letter with an accompanying list of questions on water analysis. The replies to questions affecting this special test showed that five minutes' boiling was almost the universal practise, most of the references being to the method employed by the Massachusetts State Board of Health and to Dr. Mason's book. These refer to the Kubel Method.

In a report presented at the Indianapolis meeting in 1900, the committee stated that in connection with this test "it would appear advisable to adopt a uniform procedure intermediate with the extremes now practised. Such would be afforded by the addition of the reagents to the water when cold and boiling for five minutes."[1]

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[1] Thesis at Worcester Polytechnic Institute, 1898.

Acting on this suggestion a method was drawn up, resembling the methods almost universally employed in this country. This was as follows: Treat 200 c. c. of the water or sewage *in the cold* with 5 c. c. H_2SO_4 (1-3) and 10 c. c. standard $K_2Mn_2O_8$, heat to boiling and boil for 5 minutes. Destroy the excess of $K_2Mn_2O_8$ with standard oxalic acid solution and titrate back to a pink tinge with standard $K_2Mn_2O_8$.

In the work performed with the other methods, samples of 200 c. c. were used in order that a better basis for comparison might be obtained.

Instead of relying on samples of polluted water and sewage it was thought best to make up solutions whose constituents were known and then dilute to solutions which would represent a domestic or a manufacturing sewage.

With this end in view a solution was made up as follows:

200 grains of stable droppings were digested for an hour over a steam bath with one gallon of water. At the end of that time the solution was filtered and acidulated with 50 c. c. H_2SO_4 and considerable organic matter separated out. After filtering the clear solution was put aside for use, and is spoken of as number one in the subsequent work.

For running the tests, 10 c. c. of the solution were drawn from a burette into the flask used and made up to 200 c. c. with distilled water.

The solution above mentioned would give samples representing domestic sewage, and it was proposed also to work with a known sample which would correspond to a manufacturing sewage. Another solution was made up with the same ingredients as before mentioned, and to the acidified solution were added 25 c. c. of a logwood solution; (25 grams to 200 c. c.) which is a dye stuff, and .05 gram of $FeSO_4.7H_2O$ per liter, which would represent the waste product from the wire mills. 10 c. c. of this solution made up to 200 c. c. with distilled H_2O was used as a sample.

The important methods were tried on these samples of known constitution, and the effect of the various constituents on each test was noted as far as possible. Then tests were run with samples of sewage and polluted waters, and the different methods compared.

ENGLISH OFFICIAL METHOD.

The samples tested by this method showed very uniform results. The temperature of 80° F. or 27° C. was easily maintained by the use of a water bath into which dipped a thermometer. By adding hot water from time to time, the temperature was kept constant all day. It was noticed in some of the earlier tests made that when the temperature was higher, the results invariably came higher. This was only a confirmation of the observation made by Dupre and mentioned earlier in the work.

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[1] Science, December 14, 1900, p. 910.

In the case of crude sewage, the end point was unsatisfactory in some cases. The blue color would disappear with the addition of one drop of $Na_2S_2O_3$, and then in a few minutes it would appear again, making it uncertain at times whether the test could be called complete. This led up to the tests with sewage effluent running all day and night.

Tests made every hour with the solutions of known constitution showed that the greatest decomposition of $K_2Mn_2O_8$ occurred during the first three hours. It was conclusively proven with these samples that at 80° F. the reaction was not over in four hours, though the increase was small after that time.

Brown and Fales noticed that the four-hour test was, in most cases, practically twice as much as the 15-minute test. It does not occur, however, in samples containing quantities of substances which are easily oxidised by permanganate. In the case of the second solution, the 15-minute test rose greatly in proportion, due to the readily oxidisable ferrous sulphate.

The difference in ratio between the 15-minute and four-hour tests was shown in samples of Worcester effluent from sewage which had been treated by different processes. Worcester sewage contains large quantities of ferrous sulphate from the wire mills, and the presence of this readily oxidisable body was shown by this test. Most of the sewage is precipitated with lime, but a portion goes through an open septic tank and some through an experimental closed septic tank. The septic tanks leave a considerable portion of the iron in its original state, while in the precipitation process, it is all thrown out as ferrous hydrate by the lime.

In several cases, with the English Official Method on sewage and effluents, permanent end points were difficult to obtain, and there was some doubt as to the completeness of the oxidation. Accordingly, tests were made with the effluent from the chemical treatment, running from 15 minutes up to 20 hours. The results from these tests show that the permanganate was acted upon up to 20 hours, the increase from 4 hours being about $\frac{1}{3}$. In connection with this test, a test was run with the Lawrence Method with and without $MnSO_4$, the results being respectively 1.0535 and 1.534 parts O consumed per 100,000. With the use of $MnSO_4$, the results came very near the 4-

hour English Official test and much less than the O consumed in 20 hours. This would seem to indicate that either the $MnSO_4$ corrected for more than chlorides or, that in the continued digestion with $K_2Mn_2O_8$, secondary reactions were set up which caused a decomposition of $K_2Mn_2O_8$.

The necessity of keeping an excess of $K_2Mn_2O_8$ in contact with the sample was shown in a test of several hours' duration in which the pink color nearly all disappeared. The results became very variable as time went on, though in the long run there was a gradual increase.

As compared with other methods this one gave lower results. In the case of the boiling methods the decomposition of the chlorides present in the sewage undoubtedly has its effect and higher results occur. It was shown by Brown & Fales that permanganate continued to oxidise organic matter at the boiling temperature even after thirty minutes' boiling; it was noticeable, however that at 30 minutes only 0.3 part more was absorbed than at 10 minutes.

By digesting for six hours the English Method gave results which were comparable with those obtained with the Lawrence Method, as was shown by a series of observations made on the same sample with all the methods.

THE BOILING METHOD.

The Lawrence and Worcester Methods gave results which are quite comparable, though as a rule the Worcester Method gave slightly higher results. Although the Lawrence Method gave lower results than the methods in which the boiling was continued longer, it was noticeable that the greatest amount of oxidation occurred during the first two minutes. This observation was the result of running a series of tests with the sample, in which all the methods were employed. With the samples made up from solution I., there was a greater difference between the two and five-minute boiling than in the case of samples from solution II. containing the more easily oxidisable substances.

The same facts were observed in the case of Worcester effluent from the chemical precipitation and septic tank processes. The character of these effluents has already been described.

The boiling methods require a blank, but it was noticeable that the same blank could be used for all of them. In nearly all cases the end point was very satisfactory. The solution went from colorless to pink with one drop of permanganate. With heavily polluted waters it was noticed, occasionally, that the solution would go from colorless to pink with one drop of $K_2Mn_2O_8$, and then almost immediately the pink would give place to a brown color. With such uncertain end point, it was customary to add enough $K_2Mn_2O_8$ until, on standing half a minute, there was a pink color seen in spite of the brown.

The Kubel method, in the form used in this work, *i. e.* 200 c. c. samples, was identical with the method recommended by the American Association for the Advancement of Science. Neither this latter method, nor Kubel's, nor Mason's, stated whether the solution was to be boiled before the $K_2Mn_2O_8$ was added. In the tests made, the solution was always heated to boiling before the $K_2Mn_2O_8$ was added, for it was shown in the experiments made with the method proposed by the American Public Health Association that there was a decided difference in the results, according as more or less time was taken for the cold solution and $K_2Mn_2O_8$ to come to boiling.

The method proposed by the Committee on Standard Methods of Water Analysis of the American Public Health Association was carefully tested with reference to the time for the solution to come to a boil. It was found that with a good Bunsen burner and using no asbestos, it averaged five minutes as the least time taken for the boiling to commence. It was taken for granted that not over twenty minutes would be consumed by anyone, in heating a solution from the cold to the boiling point, and accordingly a series of tests was made, regulating the temperature so that 5-10-15-20 minutes were consumed in bringing the liquid to a boil. The results were quite interesting and showed that the method gave erratic results in some cases. In the majority of cases it may be affirmed that when bringing a solution to boiling in the presence of permanganate, the longer the time before boiling begins, the greater is the amount of permanganate decomposed. The results obtained by this method, using the 5 minute test, were comparable with those obtained by the Kubel method in some cases and again they agreed more closely with those obtained by the Boston method. The three methods, however, give results which are quite comparable.

Schulze's method was found to give results which were higher than those obtained by any other method. The method is similar to Kubel's method except that oxidation is done in alkaline solution, the solution being subsequently acidified and then the excess of $K_2Mn_2O_8$ destroyed with oxalic acid. During the cooling from 100° to 50° some time elapses, and the hot solution remains in contact with permanganate for 5 or 10 minutees. This fact alone would account for higher results.

Ruppin[1] stated that in the presence of organic bodies, the presence of 800 mg. of chlorine in one liter was appreciable when boiling with $K_2Mn_2O_8$. He also stated that the influence of free HCl was weakened by the use of 1 c. c. of a 20 per cent. solution of $MnSO_4$. The influence of chlorides has always been considered an important one in these boiling methods and the methods have not been considered as accurate as the English official method with waters rich in chlorides. The Lawrence method was run with samples from known solution II, which contained .45 pts. Cl per 100,000, to which known amount of chlorides were added. A straight test was made and then a parallel test run, using 1 c. c. $MnSO_4$ 20 per cent. solution. The results showed that boiling with chlorides has considerable effect on the amount of $K_2Mn_2O_8$ decomposed, though it is noticeable that practically no more was decomposed when 80 parts of Cl were present than when .45 parts were present. This portion of the subject is worthy of more research.

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[1] Centralblatt, 1900, Bd. II, p. 1088.

In summing up the results of my investigation, the following facts appear to be warranted from the experience and figures obtained.

The English official method seems to be the most reliable as an all-round method; its results are capable of greater interpretation than those of any other method; the 15 minute and 4 hour tests give a fair idea of the quality of the oxidisable matter in the water as well as the quantity. It is adapted to waters rich in chlorides, and no blank is necessary. When employing this method, it is essential that the temperature should be kept constant. In the case of polluted waters, the oxidation is not over in four hours, and a six-hour test with this method gave results comparable with the two-minute boiling Lawrence method. At a temperature of 80° F. it was shown that there is decomposition of permanganate up to twenty hours and probably more. Whether or not this may be due to secondary reaction taking place is a point worthy of further investigation.

The Kubel and Boston methods, and the method based on the suggestion of the Water Analysis Committee when five minutes are consumed in bringing the solution to boiling, in other words, as rapidly as possible, give comparable results and have a decided advantage in being rapid methods.

The Lawrence and Worcester methods give comparable results, but somewhat lower than those obtained by the three methods just perviously mentioned.

In the boiling methods, the greatest amount of oxidation occurs during the first two minutes. A blank is always necessary and there should always be an excess of permanganate in contact with the solution in any of these tests.

Schulze's method gives very high results and cannot be compared with any of the other methods.

Ruppin's statements as to the non-appreciable effect of chlorides in the boiling test, were not borne out in this work. However, there was apparently no more decomposition of permanganate when 80 parts of Cl were present per 100,000 than when .45 parts were present. When $MnSO_4$ was not used with the boiling methods, these latter gave higher results than the four-hour English official test, and by using $MnSO_4$, the results were more comparable. The action of chlorides opens up a field for more extended research than was possible during the time at my disposal and experiments made on the subject would undoubtedly yield some interesting results.

By working with samples of the same amount, 200 c. c. as was done in this work, several of the methods are readily comparable.

In closing, I wish to state that a complete consideration of all sides of this important subject was impossible during the time at my disposal. As the work progressed, problems bearing directly on the subject presented themselves and there is yet a considerable amount of research work which can be done with profit.

The writer wishes to express his hearty thanks for the suggestions and advice given by Dr. L. P. Kinnicutt, of the Worcester Polytechnic Institute, the work carried out, being under his directions.

THE OCCURRENCE OF TUBERCLE BACILLI OF EXALTED VIRULENCE IN MAN.

By MAZYCK P. RAVENEL, M. D., Philadelphia, Pa.

(From the Laboratory of the State Live Stock Sanitary Board of Pennsylvania.)

The occurrence of tubercle bacilli in man which have a high degree of virulence for experimental animals is rare, if we can judge by the published reports, and it is still more rare to find cultures which are highly pathogenic for cattle. So marked is the difference in pathogenic power of cultures isolated from man on the one hand, and from cattle, on the other, that a division into races has been proposed depending on this feature, and Koch holds that inoculation of a given culture into cattle will show whether it is of human or bovine origin. Believing that bovine tuberculosis is a fertile cause of the disease in milk-fed children, I have for some time past taken every opportunity possible of isolating cultures of the tubercle bacillus from the mesenteric glands of those who had died of a tuberculosis which was not clearly of respiratory origin. This has led to the discovery of two cultures, one of which is as virulent for all animals on which it has been tested as cultures obtained from cattle usually are; and the other, while not nearly so virulent, is still much in advance of the usual human culture.

The material from which these cultures were isolated was furnished through the kindness of Dr. Alfred Hand, of Philadelphia.

The more virulent of these cultures, designated BB, was obtained from the mesenteric glands of a child who had died of tubercular meningitis. Two calves inoculated in the jugular vein and peritoneal cavity died in 19 and 27 days respectively, while a grown cow inoculated both in the jugular vein and peritoneal cavity died in 18 days. Examination of the lesions, both macroscopic and microscopic, leaves no doubt that the animals succumbed to a typical tuberculosis.

History of culture. Designation BB. Obtained from a child seventeen months old. Death was due to tubercular meningitis. Autopsy: Lungs normal, except posterior part of right upper lobe, which was consolidated, red on section, and had excess of connective tissue. Bronchial and mediastinal glands not enlarged. Spleen, liver and kidneys show pearly tubercles. Two feet from lower end of ileum is an ulcer 1 by 2 cm., the long diameter transverse to axis of gut. Nodules are seen on peritoneal surface. Four small ulcers, two above and two below, are found in ileum. Mesenteric glands enlarged, cheesy and some purulent. Meninges show yellow tubercles, most marked along longitudinal fissure, in choroid plexus, and over cerebellum. The mesenteric glands were used in obtaining the culture.

This case is considered by Dr. Hand the clearest one of primary intestinal tuberculosis ever seen by him.

Guinea pigs were inoculated intraperitoneally with an emulsion made from the mesenteric glands on March 9, 1901. One was chloroformed on April 19, and cultures made. Scanty growth on one tube was found June 8, and sub-cultures made.

Tests on animals. Calf 26,596, 132 pounds. Tested with tuberculin, but gave no reaction. On Dec. 4, 1901, 5 c. c. of a suspension of the fifth and sixth generation of Culture BB was injected into the right jugular vein. On the next day the temperature had risen to 103.2°. After a slight remission it rose to 103 on the seventh day after inoculation and continued to rise each day, reaching 106.4° by the fourteenth day, after which it fell slightly until death, which took place on December 21, 17 days after inoculation. The animal showed marked illness during life, the respiration reaching 68 per minute.

Autopsy. The lungs and the related glands showed macroscopically the most marked disease. Both lungs were thickly studded throughout with remarkable uniformity with miliary nodules, giving a sandy feel under the finger. The left was pneumonic, owing to the position of the calf for some time before death. Portions of the right were in a similar condition, sinking when placed in water. The bronchial and mediastinal glands were much enlarged and soft, and in the former small areas of caseation were found. The liver was large, soft and friable. Several nodules were visible on the surface, though not perceptible to the touch. The spleen showed very slight changes. The kidneys were normal.

Calf 26,597, 202 pounds. Tested with tuberculin, and gave no reaction. On December 4, 1901, 5 c. c. of a suspension of Culture BB of the fifth and sixth generation were injected into the peritoneal cavity. The temperature rose to 105.2 by the 8th day after injection, remaining very nearly the same until four days before death, when it began to fall, and reached 103 on the day before death, which took place on December 31, or 27 days from the time of inoculation.

Autopsy. Great emaciation. Loss in weight, 37 pounds. The lungs were largely involved though not uniformly. The central and posterior portions showed many areas deep red in color and solid to the feel. These were thickly studded throughout with minute grayish nodules.

The supra-sternal lymph glands were as large as goose eggs, and contained cheesy areas. The mediastinal glands were enormously enlarged, one being six inches in length by two inches in diameter.

The peritoneal cavity contained about six quarts of straw colored fluid. The peritoneum was everywhere enormously thickened, and practically converted into a tuberculous mass. In several places it was half an inch thick and fibrous. The omentum was enormously thickened also and bound over the surface of the liver. It contained a very great number of nodules, dense and fibrous, many with caseous centers. The liver was firmly adherent to the diaphragm, and one section showed many nodules. The spleen was firmly attached to the surrounding parts by a mass of partly organized fibrin, which contained many cheesy nodules. On section it appeared to be normal.

Cow 45,030, about three years old and weighing 660 pounds. On January 22, 1902, 2½ c. c. of a suspension of Culture BB of the seventh and eighth generations was injected into the right jugular vein and the same amount into the peritoneal cavity. The temperature rose to 103.2 on the second day, then fell slightly during the next six days, after which it rose steadily, reaching the maximum, 107, three days before death, after which it fell slightly, the animal dying seventeen days after inoculation. The lungs were mostly involved, both being thickly studded throughout with miliary nodules. Most of the left lung showed interlobular emphysema, while the inferior portion was much congested. On the omentum were numerous dark red spots which could be felt between the fingers. The spleen was adherent to the diaphragm, and there were many punctate hemorrhages on the surface. The liver and kidneys showed no gross changes.

A second culture obtained from the mesenteric glands of a child has shown virulence far in excess of that usually found in human cultures, though it falls short of Culture BB in pathogenic power. This culture, designated U, was isolated in December, 1900, and tested for guinea pigs and rabbits. The results, together with its manner of growing and microscopic appearance, led us to consider it a typical human culture. During the summer of 1901, it was selected for some feeding experiments on puppies, and showed a degree of virulence that was not expected. In December last, wishing to produce a slow tuberculosis in a dog, this culture was again employed. The dog grew ill rapidly, and died in January, thirty-six days from the time of inoculation. Post mortem examination revealed a typical and extensive tuberculosis, and it was decided to inoculate a calf in order to test its virulence for the bovine race. On March 4, 1902, Calf No. A45,046, weighing 74 pounds, was inoculated in the jugular vein with 6 c. c. of a suspension of culture U, of the twelfth generation, from glycerine agar, the suspension being equal in opacity to a 24-hour old culture of the typhoid bacillus in bouillon. The animal soon showed signs of illness, the breathing became rapid and labored, reaching 54 per minute. Extreme weakness and depression came on and it passed much of the time lying down. On April 19, 1902, it was killed, death seeming imminent.

Autopsy. Weight 82 pounds. Condition fair. Anterior lobes of both lungs were thickly studded with minute nodules, averaging 1mm. in diameter. Blocks from these lobes sank in water immediately. Both lungs were dense and solid to the feel throughout. The posterior lobes were air-bearing for the most part, but contained many solidified areas, which could be seen on the surface as well as on section.

The bronchial and mediastinal glands were enlarged and wet. Scrapings from cut surfaces show an enormous number of tubercle bacilli. In the mediastinal glands many yellowish nodules were seen. The pleuræ were normal. The liver was enlarged and firm to the touch. No distinct nodules could be seen by the naked eye, but throughout the substance yellowish areas were found in large numbers. The surface appeared marbled.

The spleen was enlarged and very firm, but no nodules could be detected. The kidneys showed numerous whitish areas on the surface which were found to extend quite deeply into the cortex. On section many yellowish nodules were seen. The tissues of the kidneys were much stained with bile, and the pelvis contained a gelatinous or viscous material showing the same pigment. The peritoneum was normal.

Microscopic Examination. The solidified portions of the lungs contained large areas which are not air-bearing, the alveoli being completely filled with leucocytes and epithelium or epithelioid cells. The small bronchi are filled by an exudate made up largely of leucocytes. The central portions of the nodules stain poorly, and fragmentation of the neuclei of the cells is seen, but no distinct caseation can be found. In sections stained with carbol-fuchsin innumerable tubercle bacilli are seen. In other parts of the lungs which are still largely air-bearing, minute nodules are found in large numbers, having the same general characteristics as those just described. Throughout the sections many tubercle bacilli are seen, and in the nodules they are in clusters. In the liver no typical tubercles are found, but there are many minute nodules made up of round cells, and on the borders of these a considerable number of giant cells with peripheral nuclei occur. Sections stained with carbol-fuchsin show large numbers of tubercle bacilli clustered in these areas.

The spleen shows round cell infiltration, giant cells and many tubercle bacilli.

The white areas in the kidney described above show a round cell infiltration, and large numbers of tubercle bacilli. The bronchial glands show areas of beginning caseation, a few giant cells, and innumerable tubercle bacilli.

This calf showed a well marked tuberculosis, proving that the culture with which it was inoculated had a degree of virulence much above that usually found in cultures of human origin. Our experience with this culture leads us to believe that the usual method ot employing only guinea pigs and rabbits in testing the virulence of the tubercle bacillus does not always give entirely conclusive results. Unfortunately, the expense attending the use of larger animals hinders their more general employment.

History of Culture U. Obtained from a child three years of age, whose death was due to tubercular meningitis. Autopsy: Lungs and bronchial lympathic glands full of miliary tubercles. Anterior mediastinal glands much enlarged. Spleen extensively involved, liver less so. On abdominal surface of diaphragm were a number of flat, yellow nodules. Mesenteric glands enlarged, and contained old, yellow, cheesy nodules. One small tubercle found in right suprarenal. Purulent and cheesy nodules found in meninges and encephalon. The culture was obtained from the mesenteric glands.

The pathologist, Dr. Hand, was unable to decide with certainty as to the origin of this case.

In seeking to interpret these findings we are confronted with one of two propositions: Either the human tubercle bacillus at times has a pathogenic power fully equal to the bovine type, or on the other hand, the bovine bacillus is found in the digestive tract of certain milk-fed children. The morphology, manner of growth and virulence of Culture BB, and the history of the child from which it was isolated appear to me to leave little doubt that this child was a victim of the bovine tubercle bacillus, derived in all probability from milk taken as food.

ABSTRACT OF A PAPER ENTITLED TUBERCULOSIS IN CATTLE INDUCED BY SUBCUTANEOUS INOCULATION OF MATERIAL OBTAINED FROM A CHILD.

By E. A. de SCHWEINITZ, M. D., Washington, D. C.

Dr. de Schweinitz presented the history of a case of generalized tuberculosis in a child of five years of age, that had been brought up upon milk. The cultures obtained from the mesenteric glands of this child had produced generalized tuberculosis in a heifer after subcutaneous inoculation, within about a month. Drawings which showed the appearance of the lung from this calf and also the appearance of the liver of a pig, both of which had been submitted to subcutaneous inoculation with this germ, were shown. In addition drawings showing the comparative results of a subcutaneous inoculation of bovine and human tubercle bacilli in monkeys were presented. These indicated that the bovine tubercle bacilli were very much more virulent for the monkeys than the human tuberculosis bacilli used.

Attention was also called to the fact that as the precipitines seemed to be produced interchangeably in monkey blood and human blood, there was evidently a close relationship between these two species of animals, and that therefore the conclusion was a fair one, that if the monkeys were so susceptible to germs of bovine origin, man in all probability was equally susceptible.

In the discussion which followed this paper, Dr. de Schweinitz further stated that the cultural characteristics of the germ which had produced the tuberculosis in the heifer upon subcutaneous inoculation, appeared to be those which some authors claim to be possessed only by the bacilli derived from the bovine species, and that further, whether the germ that killed the heifer was regarded as a bovine germ or a human germ, the conclusions naturally were of equal value, as if the germ was of bovine origin then it seemed that tuberculosis in children could be produced by bovine bacilli. If, on the contrary, the germ was what is commonly called the human germ, then it was a germ which was virulent for cattle. The fact was also mentioned by Dr. de Schweinitz, that the bovine which was used in connection with his work, was no longer as virulent for animals as it had been. The original virulence had apparently been diminished through long cultivation upon liquid artificial media. All of the facts observed, therefore, seemed to show that the virulence of both human and bovine bacilli is largely influenced by their growth both inside and outside of the animal body.

A REPORT OF THE BACTERIOLOGICAL AND PATHOLOGICAL FINDINGS IN A FATAL CASE OF DYSENTERY AT TUCKAHOE WITH ADDITIONAL NOTES UPON THE SERUM REACTIONS OBTAINED WITH THE BLOOD FROM A NUMBER OF THE DYSENTERY CASES.

By WM. H. PARK, M. D.,
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And H. W. CAREY, A. B., M. D.,
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An extensive epidemic of dysentery occurred in and around Tuckahoe during the past summer. Cultures were made at the time from a fatal case (Carey) and later the agglutination characteristic of the culture studied and the serum reactions tested in a number of the convalescent cases (Park).

Although the information here gathered is very limited in amount, yet it is published because the epidemic has ended and it is thought that the results obtained add something of value to the information already gathered on the subject of the relation of a rather ill-defined group of bacilli to epidemic dysentery.[1]

The following are some of the most essential points concerning the epidemic. Until July, 1902, there had been very little dysentery in Tuckahoe (a town of some 3000 inhabitants), but during July, August and September of that year there were about three hundred and fifty cases, with a mortality of about six per cent. The average age in the fatal cases was a little less than three years. The epidemic was most severe among a class of people living in filthy surroundings. There was in this region no system of sewerage, the old-fashioned out-of-door closets being in general use. Chemical examination showed the well water to be badly contaminated. The epidemic in a less severe form spread to the districts adjacent to Tuckahoe where the water supply and sewerage was good.

The chief points of clinical interest in regard to the cases as summarized by one of the physicians (Dr. B. K. Tupper), who treated over one hundred and fifty persons, are as follows:

The onset in all of the cases was sudden. The disease was ushered in by cramps, diarrhea and tenesmus. The stools were at first feculent, then sero-mucous, then finally bloody and mucous. Blood usually appeared on the second day, was present in considerable quantity for two or three days and then disminished. It disappeared entirely in most cases in six or ten days.

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[1] A full account of the territory covered by the epidemic and of the clinical course of the disease in the cases can be found in the New York University Bulletin of the Medical Sciences, Vol. 2, No. 4, October, 1902, by Park and Dunham.

Temperatures were low, if there was no complication the average case ran from 99 degrees to 101 degrees F., for one or two days, then fell to normal or sub-normal. Sub-normal temperatures were very frequent. Tenesmus was a marked feature of almost every case. A few cases developed rectal prolapse. The stools had the characteristic cadaveric odor, and were composed of mucous and blood, frequently mixed with fragments of mucous membrane. A stool frequently seen was composed of "coffee ground" sediment, serum and mucous. The stools usually consisted of a small amount of mucous frequently streaked with blood. Pure bloody evacuations occurred in most of the cases seen by me. These discharges only lasted twenty-four to forty-eight hours as a rule, beginning on the second day. Mild cases in general lasted two weeks, severe three to four weeks, malignant a few days. Convalescence was slow almost without exception.

The autopsy records of the fatal case from which the dysentery bacillus (Shiga type) was obtained, are as follows:

Harriet T. Age one year and four months. Died August 26th, at 7 A. M. Autopsy 1 P. M. Service of Dr. Oliver Austin, Tuckahoe, N. Y.

Mucous Membranes—Pale. Edema of feet and legs.

Peritoneum—On opening abdominal cavity both layers of peritoneum are smooth. No adhesions. No distention with fluid.

Intestines—Not discolored.

Small Intestines—Slightly distended. Mesenteric glands large and red, vary in size from 3 to 6 mm. in diameter.

Large Intestines—Outer surface vessels congested and prominent. On section covered with a yellowish mucous. Mucous membrane seems to be absent in places. Solitary follicles are elevated and enlarged, especially in the region of sigmoid flexure. In some instances the center of the follicles are depressed and appear to be necrotic.

Appendix—No adhesions on the outer surface. On section lymphatic follicles are swollen with depressed centers similar to the condition described in the large intestines.

Small Intestine—Peyer's patches are distinctly swollen, but in no instance is there ulceration or necrosis.

Kidneys—Fibrous capsule not thickened and peels easily. On section cortex not thickened. Pyramids distinct. Malpighian corpuscles not visible. Blood vessels congested. Kidney substance bleeds easily.

Liver—Outer surface smooth. Consistency soft. Capsule not thickened. On section lobules are distinct.

Spleen—Normal in size. Capsule not thickened. On section malpighian corpuscles distinct. Pulp not increased. Bleeds easily on section.

Anatomical Diagnosis. Passive congestion of the lungs, spleen, kidneys and liver. Follicular enteritis. Hyperplasia of the mesenteric lymph glands.

MICROSCOPICAL EXAMINATION.

Liver—Cells of parenchyma normal. Marked congestion of the hepatic vein and capillaries at center of lobule.

Spleen—Slight congestion.

Heart Muscle—Normal.

Kidney—Cloudy swelling.

Lung—Marked congestion of lung capillaries.

Thymus—Marked congestion with considerable accumulations of eosinophilic cells, mostly polynuclear.

Large Intestine—Mucosa. Glands are for the most part normal, but over the solitary follicles they have broken down somewhat and contain polynuclear leucocytes. The interglandular stroma in these places has undergone necrosis. The necrotic areas extend down into the submucosa in the region of the solitary follicles. The blood capillaries of the solitary follicles are much dilated and congested. The submucosa is thickened and slightly oedematous. The connective tissue cells appear to have undergone a slight hyaline degeneration. The musculature is not affected, neither is the peritoneal coat.

Small Intestine—Normal.

Large Intestine—In section strained by Flexner's method bacteria were demonstrated in the necrosed areas which corresponded in shape and size to those of the colon group.

AUTOPSY BACTERIOLOGY.

Cultures were made from the lungs, heart, liver, spleen, kidneys and large intestines.

Heart, Spleen and Kidneys—Sterile after 72 hours' incubation.

Liver—B. Coli Communis.

Large Intestine—A series of twelve agar plates were made and placed in the thermostat at 37° for 24 hours. At the end of that time all the colonies were marked and cultures from those chosen for inoculation subsequently proved to be B. Coli Communis.

After a second incubation of 24 hours new colonies developed, some of which were inoculated into fermentation tubes containing glucose bouillon.

After 24 hours several tubes contained gas and the bacilli in them proved to be colon bacilli. The culture in one tube, however, developed no gas after 48 hours, and subcultures were made upon different culture media.

The cultural characteristics of this organism are as follows:

Agar—Slight growth along line of needle, white, opaque, elevated and moist.

Milk—Slight acid formation in first two days. Alkalinity developed after ten days.

Neutral Red—No change.

Bouillon—Clouded.

Dunham's Peptone Solution—Very slight indol formation at end of five days.

Gelatin Bouillon—No gas.

One per cent. Mannit in Peptone Solution Agar—No production of acid in deep stab or surface growth.

Gelatin—Not liquefied.

Glucose Bouillon—No gas.

Saccharose Bouillon—No gas.

Morphology—Short bacillus, rounded ends. Sometimes in pairs.

By Gram—Decolorized.
Motility—Very slight, if present at all.
Flagella—Not demonstrated.

Agglutination Reactions.—The reaction of the Tuckahoe bacillus with the serum from a horse, which had received seven subcutaneous injections of Flexner's Baltimore culture, compared with Shiga's Japan culture and Flexner's Baltimore culture is as follows:

Table showing agglutination of Tuckahoe culture and of example of Shiga and Flexner Baltimore types in horse immunized to Baltimore culture:

Dilutions	1-50	1-100	1-250	1-500	1-1000	1-2000	1-4000
Shiga Bac.	++	+	+	—	—	—	—
Flexner's Baltimore bacillus	++	++	++	++	+	—	—
Tuckahoe Bacillus	++	+	+	—	—	—	—

++ = Complete clumping reaction; + = incomplete.

In one sense all these cultures gave a positive serum reaction and the difference between the reactions of the Flexner and the Shiga and Tuckahoe cultures in this serum appears from these results to be a quantitative rather than a qualitative one. That this is probably not true is shown by the fact that this horse's serum clumped all three cultures in 1-250 dilutions before receiving injections. A young goat's serum which before injection with cultures did not agglutinate any of the varieties of dysentery bacilli in our possession gave the following results after seven injections with a bacillus which agreed with the indol Baltimore culture type in agglutination reaction, but differed in certain cultural characteristics.

Table showing agglutination reaction in a serum having only specific agglutinations:

Dilutions	1-50	1-100	1-250	1-500	1-1000	1-2000	1-4000
Shiga Bac. Flexner's New Haven Tuckahoe	—	—	—	—	—	—	—
Flexner's Manilla Baltimore, etc., Seal Harbor and N. Y. Cultures	++	++	++	++	++	+	—

++ = Complete agglutination; — = no agglutination of bacilli.

It is a striking fact that all the varieties of dysentery like bacilli obtained from dysentery cases which produce indol in large amount and develop acid from mannit, agglutinate alike, while those which produce no indol or only a trace and do not develop acid from mannit vary more or less from them.

The Serum Tests of the Blood of Convalescent Dysentery Cases From Tuckahoe.—Information concerning the extensive epidemic of dysentery only reached one of us on his return to New York in October. No acute cases remained and no dysentery-like bacilli were found in feces of several convalescents examined.

The finding of the bacillus of the Shiga type in a fatal case being then unknown, the serum tests were carried out with the Flexner Manilla type, to which type the bacilli obtained by us in New York City belonged.

The serum reaction was positive within one and one-half hours in a 1-50 dilution in thirteen out of twenty-two cases. The agglutinating strength shown was much above the average found by us in the serum of those not suffering from dysentery. Tests made two months later from some of the same cases showed a marked reduction in the agglutination strength of the sera of these cases. Although the results of these tests are insufficient to prove that the mannit fermenting type of bacilli were present and active in these cases, still they strongly suggest this to be so.

If this conclusion is correct, then bacilli of both the mannit fermenting and Shiga type were present in the cases. The Shiga variety was shown to be present by finding it in a fatal case and the other's presence was suggested by the positive serum reactions obtained with the Baltimore culture in serum from the dysentery cases after their recovery. This proof was strengthened bv the rapid disappearance of the agglutinin as shown by tests made two months later. When tested at this time the reaction between both varieties of bacilli and the sera was nearly equal in amount.

SUMMARY.

From a fatal case of dysentery occurring in a severe and extensive epidemic in a small town situated just north of New York City a bacillus was obtained which in all but one of many cultured tests seemed identical with Shiga's Japan and Flexner's New Haven culture. The only point of difference being that the Tuckahoe culture produced a trace of indol in peptone solution. The specific agglutinins developed in animals through injection of the group of "dysentery" bacilli which produces considerable indol and develops acid from mannit does not agglutinate the Tuckahoe, Shiga or New Haven cultures. All bacilli so far obtained in America outside of the New Haven and Tuckahoe cultures seem to belong to this class. (Flexner's Baltimore and Philadelphia cultures and ours from New York City and Seal Harbor.)

The serum reactions of the blood taken from a number of convalescent cases with the Baltimore culture were on the average much more marked than those obtained by us from the blood of acute cases of other intestinal diseases and indicate that this variety of bacilli, as well as the Tuckahoe variety, was present and active in many of the Tuckahoe dysentery cases. Three months later the serum reaction had largely disappeared in the cases tested and was about alike for both varieties of bacilli.

CONCLUSIONS.

At least two distinct varieties of bacilli are found in the intestines of those suffering from dysentery and colitis which have not as yet been met with as a rule in other conditions. These two varieties differ from one another in certain cultural characteristics (indol formation and mannit fermentation) and also in their agglutination reaction.

Different cultures obtained from widely separated places though agreeing in the cultural tests so far tried and resembling each other greatly in their agglutinating reactions with most sera, yet according to results obtained by us differ from each other when treated with sera from various animals.

Though Martini and Lentz are right in their statement that the Shiga type differs from Flexner's Manilla Baltimore type in cultural and agglutination reaction, yet they appear to us to be wholly wrong in concluding upon our present information, that one type of organism is any more a factor in dysentery than the other. Indeed, bacilli such as those obtained by Deycke from dysentery in Constantinople which approached the colon type in cultural characteristics cannot as yet be overlooked.

It seems to us that it is extremely probable that Shiga, if he carries his cultural and agglutination tests as far as is necessary to differentiate the different varieties will find that in Japan as well as in America and elsewhere, not one, but several varieties are present in the intestines of different and perhaps the same patients.

The use of more accurate agglutination tests in connection with additional culture media has brought to light the fact that in different epidemics the bacilli believed to have excited the disease vary so greatly from the original culture of Shiga that at present we can hardly decide whether a single group is broad enough to comprise them all.

In closing we wish to express our sincere thanks to Dr. Simon Flexner for his kindness in sending us his cultures and to Dr. Katherine R. Collins for assisting in making the numerous tests required.

OBSERVATIONS ON THE MORPHOLOGY OF B. DIPHTHERIÆ, B. PSEUDO-DIPHTHERIÆ AND B. XEROSIS.

By FRANCIS P. DENNY, M. D., Brookline, Mass.

INTRODUCTION.

The difficulty of distinguishing B. diphtheriæ by its morphology from B. pseudo-diphtheriæ and B. xerosis lies in the great variation in form which these bacilli present. Thus while B. diphtheriæ is readily distinguished from B. pseudo-diphtheriæ when the characteristic bipolar granules are present, the fact that they may both show similar solid staining forms has led many to believe that these two forms are inseparable.

The great variations in form of B. diphtheriæ which have been described, and which have been represented by Wesbrook[1] in his plates, would seem to point to the fact that the diphtheria bacillus is pleomorphic. The question of the occurrence of pleomorphism in bacteria is an important one. By pleomorphism is not meant the variations in form which bacteria show when growing on different media or under other varying conditions. Nor do such changes as take place in the different stages of development of bacteria constitute pleomorphism. According to Migula[2], pleomorphism in this restricted sense does not occur in the lower bacteria.

Zopf,[3] who has many followers, claims that we must admit the occurrence of pleomorphism if we include the groups of the higher bacteria, leptothrix, cladothrix and streptothrix. Migula, however, points out that most of the variations seen in these higher bacteria represent different stages in their development or result from varying conditions of growth.

It has seemed to the writer that the diphtheria bacillus is pleomorphic, if at all, only in so far as it has a stage of development like that of the higher bacteria, and that most of the changes in form represent merely different stages in its development or result from varying conditions of growth.

The work here reported was undertaken to determine what are the changes in morphology which take place in the diphtheria bacillus and allied forms during their growth on blood serum, and also what are the effects on their morphology of certain variations in the conditions of growth, such as temperature, reaction of media and symbiosis.

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[1] Wesbrook. Transactions of the Association of American Physicians, 1900.

[2] Migula. System der Bakterien, 1897; Band I., p. 212.

[3] Zopf. Reference: ibid. Band I., p. 219.

The development of the diphtheria bacillus is so rapid that within the first fifteen hours the bacilli may undergo marked changes in their morphology. These changes can only be appreciated by the study of preparations made at intervals from the same culture, beginning after a few hours of growth.

B. DIPHTHERIAE.

Ten pure cultures of B. diphtheriæ from clinical cases and of proved virulence to guinea pigs were used. Emulsions of a small amount of growth of 24-hour serum cultures were made in distilled water. Serum tubes were inoculated by rubbing a loopful of this emulsion thoroughly over the surface. The culture was incubated at 36° C. and preparations were made from each at the end of 4, 8, 12, 15, 24, 36, and 48 hours.

Four-hour cultures. There is no visible growth on the serum at this age, but many bacilli are found in the preparations. The bacilli are solid staining forms, frequently appearing to be paired (C^2 and D^2).[1] There is considerable variation in size and shape, some rods being decidedly clubbed. The irregularity at this age is probably to be explained by the fact that these bacilli are daughter cells of the club-shaped bacilli in the 24-hour culture from which the tube was inoculated. This irregularity is even more marked in two and three hour cultures, and becomes gradually less with continued cell division.

Eight-hour cultures. The growth is usually just visible. The bacilli here also are solid staining, more commonly arranged in pairs parallel to each other and of more uniform size than in the 4-hour cultures. Occasionally the ends of the bacilli stain faintly and small granules may appear, but the granules are not common in pure cultures at this age except where a very vigorous growth has become apparent on the surface.

Twelve-hour cultures. The visible growth at this age is usually marked. Microscopically the bacilli are longer. Many of them stain faintly toward their ends and often at the tip of the lightly stained portion a small, deep granule is seen. In other cultures the granules appear in short, even-stained forms with rounded ends. As a rule, however, the typical granular forms (C and D) are not common in pure cultures of this age.

Fifteen-hour cultures. The changes noted above have become more marked. The bacilli are still longer and occasionally larger at one end (clubbed). The granules are larger and more abundant. The solid staining forms are relatively fewer.

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[1] The letters here and in other parts of the paper refer to Wesbrook's types.

Twenty-four hour cultures. At this age the solid staining forms are very few in number or wholly absent. The bacilli are longer. The paired arrangement may still sometimes be seen, but the individuals of the pair are both much longer and swollen at their distal ends (clubbed). Most forms show granules; the long, clubbed forms present small granules, while the short bacilli of even contour have larger ones. Instead of granules some of the bacilli may show protoplasm broken up into segments, giving a barred appearance, although seldom so regular as the barred forms of B. xerosis.

Thirty-six-hour cultures The bacilli are longer and the appearances noted in the 24-hour cultures have become more marked. Many of the bacilli may begin to take the stain poorly.

Forty-eight-hour cultures. Many of the bacilli stain very poorly. Occasionally, where the colonies are not confluent, the bacilli may grow out into even longer forms (filaments).

The age of the cultures at which these different changes take place vary considerably. Thus at times granular forms may be present in 8-hour pure cultures, while again their appearance may be delayed till 15 hours. The nearness of the colonies has a marked influence. Where the colonies are confluent the granular forms appear earlier than when the colonies are discrete. Different strains of B. diphtheriæ also give different results. Thus some will grow much more rapidly and vigorously on blood serum than others. In the vigorous cultures the different stages of development are passed through more quickly than in the weaker growing forms. In some strains large granules develop in short even bacilli with round ends and have little tendency to grow out into the long clubbed forms. Others have small granules, develop in bacilli with faint staining pointed ends and later develop into long clubbed forms.

To recapitulate: In the growth of the diphtheria bacillus on serum, we find during the first eight or twelve hours solid staining bacilli. The growth during this time is characteristic of all bacilli and consists in elongation of the rods and binary fission.

With the appearance of growth on the serum, there begins a series of changes in the individual bacilli. They elongate without fission, metachromatic polar granules appear, one end of the rod may become swollen, the protoplasm may break up into segments, long filaments may be formed, and rarely also true branching occurs. All these changes are characteristic of the higher bacteria and especially of the streptothrix.

From the above it is seen that the development of the diphtheria bacillus may be divided into two distinct stages. In the first stage we have the organism multiplying, as all bacilli do, by elongation and fission, and to this might be given the name "*solid staining*" or "*bacillary stage*." In the second, we have a "*higher stage*" of development where changes take place in the individual organisms similar to those seen in the streptothrix.

The conditions which bring about the transition from the solid staining or bacillary stage to that of the higher development appear to be dependent upon the accumulation of the bacilli or the products of their growth upon the serum. The transition usually begins with the appearance of growth on the surface. It appears earlier if the colonies are near together. The conditions for the transition appear to be somewhat analogous to those of spore formation.

EFFECT ON THE DEVELOPMENT OF B. DIPHTHERIAE OF CERTAIN VARIATIONS IN CONDITIONS OF GROWTH.

VARIATIONS IN TEMPERATURE.

Growth at room temperature (19°-21° C.). At the end of 24 hours there is, as a rule, no visible growth. The preparations show solid staining forms similar to four to eight-hour growths in the incubator. At the end of 48 hours the growth is usually just visible and we find microscopically a few granular forms. In 72 hours the growth is usually abundant and the granular forms are well marked. The bacilli are short with round ends and large granules. As is always the case where bacilli of this kind predominate, there is less tendency for the bacilli to grow out into long or clubbed forms.

Growth at 40° C. This also has the effect of delaying the appearance of granules, and macroscopically growth appears somewhat later. Fifteen and twenty-four hour cultures may show only large, thick, solid staining forms.

REACTION OF MEDIA.

Acid serum + 2 per cent. Serum with a reaction of + 2 per cent. and upwards delays the growth of the bacilli. Only solid staining forms may be found in 15 to 24-hour cultures. After 48 hours the bacilli are short and sometimes oval in shape, with well-marked granules.

Alkaline serum — 2 per cent. The effect of this is less marked, although the solid staining stage is usually somewhat prolonged. In some cultures there seems to be more tendency for the bacilli to show barred forms than on ordinary serum.

SYMBIOSIS.

The changes which have been described take place in pure cultures. The diagnosis of diphtheria is made with mixed cultures from the throat, hence the importance of studying the effects of symbiosis.

Growth with staphylococci. The staphylococci are the most common forms found in throat cultures. The staphyloccous pyogenes aureus was used in combination with each of the ten diphtheria cultures. An emulsion in distilled water was made of the staphylococcus and then mixed with the emulsions of B. diphtheriæ in varying proportions. From these mixed emulsions, serum tubes were inoculated and preparations were made at intervals.

It is found that there are difficulties in getting bacteria to grow together in a uniform way, and especially to regulate the relative amount of growth of each. In cultures, however, which showed satisfactory growths of each organism, certain effects obtained were quite constant. The effects produced were in part dependent on the relative number of cocci and diphtheria bacilli. If there were only a few cocci, the growth of the bacilli seemed to be rapid, and granular forms appeared in 8 hours, even earlier than in pure cultures. If the relative number of cocci were very large, the granular forms were not found so early, often appearing later than in pure cultures. In rare instances, 15-hour cultures in which there were only a few bacilli and many cocci showed only a few solid staining forms.

Growth with streptococci. The growth of the streptococcus during the first 8-12 hours is very rapid, but soon ceases. A few streptococci growing with the B. diphtheriæ hasten very much the appearance of granules. Relatively large numbers of streptococci were never observed to retard their development.

The granular forms in mixed cultures are typical even forms with round ends and large granules (C & D). There is less tendency for the growth of long, clubbed forms than in pure cultures.

To recapitulate: The variations in the conditions of growth which have been studied—high and lower temperature, acid and alkaline media, and symbiosis—have all, with one exception, resulted in delaying the development of the bacilli and in prolonging the sold staining stage. The exception is the growth of B. diphtheriæ with a *few* staphylococci or streptococci, under which conditions the granular forms may appear even earlier than in pure cultures. All of these changed conditions, except perhaps the alkaline media, have the effect of producing short, even forms, with large granules and with less tendency to growth into the long clubbed forms.

The observations which have been made of the effect of symbiosis on the morphology of B. diphtheriæ seem to throw some light on the observations of Wesbrook[1] and Gorham,[2] that during convalescence there is a tendency for the solid staining forms to replace the granular forms. At the time when cultures are made for diagnosis and also when the first cultures "for release" are made, there are relatively many diphtheria bacilli and few of the other throat bacteria. The conditions are thus the most favorable for the early and characteristic development of granular forms. As convalescence advances, the diphtheria bacilli become fewer and the throat bacteria, whose growth had perhaps been somewhat checked during the disease, now become more abundant. We thus have the conditions for a later development of granular forms, and in the 15-hour cultures for diagnosis the solid forms will be more abundant.

B. PSEUDO-DIPHTHERIAE.

Thirty-five cultures of this bacillus have been isolated by the writer. All have produced alkali in sugar bouillon except three, which did not affect the reaction of the media at all, and all have caused clouding of the bouillon. None of them were virulent to guinea pigs.

The cultures of all ages have shown solid staining forms (C^2 and D^2). The bacilli are very similar to the young solid staining diphtheria bacilli, but, as a rule, they are shorter, thicker and have more rounded ends. When individuals are found isolated as in mixed cultures they are absolutely indistinguishable from the young diphtheria bacilli.

The most marked and distinguishing characteristic of the B. pseudo-diphtheriæ is that instead of growing into long forms in the older cultures the bacilli *are actually shorter* in 24 and 48 hour cultures than they are at the age of 12 hours.

Six cultures of B. pseudo-diphtheriæ have been studied in serial preparations and under varying conditions as was done with B. diphtheriæ. This has shown that B. pseudo-diphtheriæ presents no marked changes in its morphology at different ages and under varying conditions. No long forms and no branching were observed.

It is thus seen that B. pseudo-diphtheriæ has a development like the young B. diphtheriæ, but no higher development like that of the older diphtheria bacilli and of the streptothrix.

As B. pseudo-diphtheriæ and young B. diphtheriæ are indistinguishable when seen in mixed cultures it may be impossible to make an immediate diagnosis of a 15-hour culture in which only solid forms are found. In the writer's experience, *longer incubation will always settle the question.* If the bacilli are true diphtheria, granular forms will appear in 24 hours. If no granules are seen it will be found that the bacilli when isolated have all the characteristics of B. pseudo-diphtheriæ.

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[1] Wesbrook. Transactions of the Association of American Physicians, 1900.

[2] Gorham. Journal of Medical Research, 1901, Vol. VI, p. 201.

If there are many solid forms in a 15-hour culture and no granular forms, it is probable that they are the pseudo-diphtheria bacilli, because as we have seen, when there are many B. diphtheriæ in mixed cultures the granules appear early. If only a few solid staining bacilli are present they may be true diphtheria bacilli in which, as frequently happens, under such circumstances, the granules have appeared late. Such a culture should be reported as doubtful and incubated longer.

B. XEROSIS.

Several varieties of bacilli which resemble the diphtheria bacillus have been found by us in the eyes of healthy persons. The one studied has resembled the diphtheria bacilli the most and has corresponded with the description of B. xerosis given by Migula.[1]

Eight cultures have been studied in the same way as B. diphtheriæ. None of them showed virulence for guinea pigs.

Eight and twelve-hour cultures show solid staining bacilli very similar to young B. diphtheriæ and B. pseudo-diphtheriæ.

Fifteen-hour cultures show slightly longer rods. In addition to the single break in the protoplasm seen in the younger forms a suggestion of a light staining area in either half may appear, which is the first indication of segmentation. Very rarely short granular forms are seen.

Twenty-four-hour cultures. The bacilli are longer. The barred or segmented appearance is more marked. A few granular forms may be seen, but they are not usually characteristic.

Forty-eight-hour cultures. The bacilli may be longer and show more marked segmentation, depending upon the amount of development in twenty-four hours. A few forms may have begun to take the stain faintly and are breaking up.

VARIATIONS IN THE CONDITIONS OF GROWTH.

At room temperature there is no growth of the B. xerosis if recently isolated.

Acid serum has the effect of delaying the growth and of causing granular instead of barred forms to appear. The bacilli are short and thick and may have good-sized granules.

Symbiosis. The effect of the growth in mixed cultures with staphylococci and streptococci is to increase the number of granular forms in 15 and 24-hour cultures. No barred forms at all may be present and the bacilli may resemble very much B. diphtheriæ. As a rule, however, the granular forms of B. xerosis are thicker and shorter and the granules are less sharp. An important means of distinguishing them is by Neisser's stain by which only an occasional granule will be stained, while in cultures B. diphtheriæ showing granular forms, with Löffler's blue, a large proportion of the bacilli will show granules by Neisser's method.

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[1] Migula. System der Bakterien, 1900. Band II, p. 501.

To recapitulate: B. xerosis in young cultures shows solid staining bacilli like young B. diphtheriæ and B. pseudo-diphtheriæ. Like the B. diphtheriæ it has a higher stage of development. This is characterized by elongation of the rods and the breaking up of the protoplasm into segments. Growth on acid media and symbiosis with staphylococci and streptococci causes a development of granular forms which are very similar to B. diphtheriæ.

RELATIONSHIP OF THESE THREE SPECIES.

This work has shown that these three species are indistinguishable morphologically at certain stages and under certain conditions. All of them, however, in the cultures which have been studied have shown characteristics sufficient to separate them when all stages of their development have been studied.

NOTES ON MORPHOLOGY.

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I. "SNAPPING" AS A CHARACTERISTIC OF THE DIPHTHEROID GROUP.

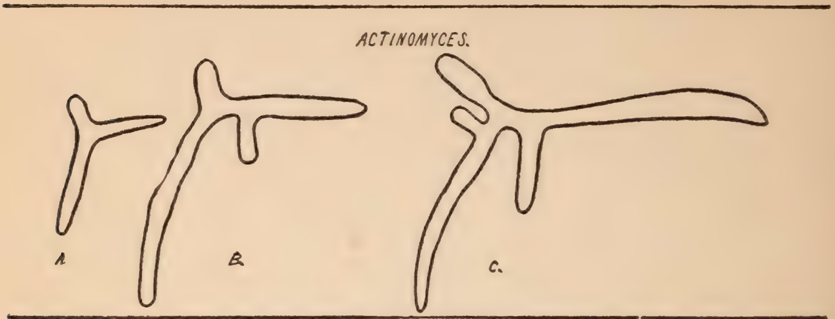
The post-fission movement, conveniently called "snapping," characteristic of typical *B. diphtheriae* during multiplication, although described in 1898 by Kurth (Zeit. f. Hyg.), by Nakanishi in 1901 (Cent. f. Bakt.) and found independently by Hill in the same year, has received little attention. In order to determine whether or not this striking peculiarity existed in species other than typical *B. diphtheriae*, the following bacteria were examined in the hanging block in this laboratory:

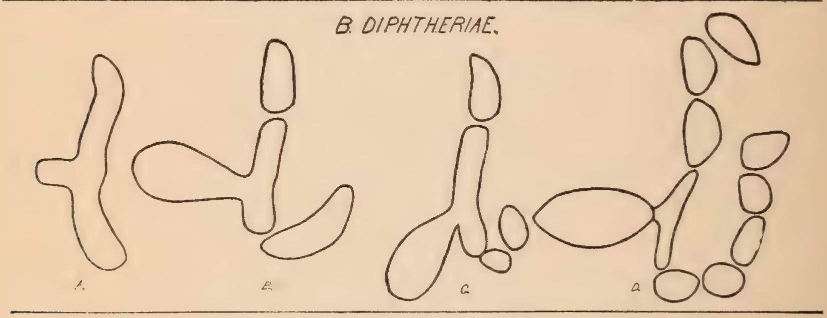
Bacterium.	Source.	Observed by.
B. anthracis	Rettger	Rettger
B. megatherium	M. I. T.	Rickards
B. megatherium	Rettger	Rettger
Sp. choleræ Asiaticæ	Rettger	Rettger
B. coli	Boston	Rickards
B. ramosus	M. I. T.	Rickards
B. ramosus	Rettger	Rickards
B. indicus	Rettger	Kendall
B. fluoresc. non-liq.	Rettger	Kendall
B. xerosis	Denny	Hill
B. pseudo diph.	Denny	Hill
B. typhi alsd.	Wilson	Hill
B. pestis	Wilson	Hill
B. muc. caps.	Rettger	Hill
B. pyocyaneus	Rettger	Hill
B. of logwood	Rickards	Hill
Whipple's "C"	Whipple	Hill
Whipple's "D"	Whipple	Hill

Owing to interruptions, no further species were examined. In none of the above species, except in *B. xerosis* and *B. pseudo-diphtheriae*, do snapping movements exist. Dr. Hefferan's rosette-forming bacillus (Cent. f. Bakt., 1902), from the author's description, seems to perform this same kind of movement, but only after definite fission, very slowly, and without the initial snap. The writers of this paper believe that the "snapping" movements sharply distinguish *B. diphtheriae*, *B. pseudo-diphtheriae*, *B. xerosis*, etc., on the one hand from the "typical bacilli" on the other.

II. BRANCHING OF THE ORGANISM OF ACTINOMYCOSIS.

A free growing culture was obtained from the Carnegie Institute through Dr. Rettger, of Yale. Hanging block preparations observed during 48 hours yielded results shown by the accompanying plate (simple budding).





DESCRIPTION OF PLATES.

Branching in Actinomyces compared with branching *B. diphtheriae.*

Fig. 1 shows branching in Actinomyces.

Time relations: A to B 140 minutes; B to C 24 hours.

Fig. 2 shows branching in *B. diphtheriae.*

Time relations: A to B 85 minutes; B to C 8 minutes; C to D 137 minutes.

Both organisms were observed in the hanging block.

A PROPOSED CLASSIFICATION AND METHOD OF GRAPHICAL TABULATION OF THE CHARACTERS OF BACTERIA.

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Great difficulty is experienced by bacteriologists in establishing the identity of a bacterium whose morphological, cultural and biochemical characters are known.

This is due to the following facts:

1. Inaccessibility of the literature.
2. Incompleteness of descriptions and indefiniteness of terms used.
3. Lack of uniformity in methods and choice of media.
4. Want of an homogeneous classification, in which the groups are well defined.

In this paper the following order will be observed:

A. Discussion of the generally accepted classifications with reference to the above points.

B. The basis for a complete, homogeneous classification.

C. A detailed account of such a system.

D. A method for recording graphically the characteristics of bacteria in such a system.

A. DISCUSSION OF CLASSIFICATIONS.

Of the many classifications in use at the present time, three only need be considered in this connection, namely those of Migula (1), Fuller and Johnson (2), and Fischer (3).

These have been chosen because they are the most generally accepted, and because each is based upon some very important factor which is not common to the other two.

Of the three classifications, that of Migula is the best known. It is based upon morphology, the morphological groups being derived as follows: Coccaceæ, arrangement of the cells with reference to the planes of fission, and the presence or absence of flagella; Bacteriaceæ, presence and arrangement (or absence) of flagella; Spirillaceæ, flexuosity of the cell, presence and arrangement (or absence) of flagella; Chlamydobacteriaceæ, presence or absence of a sheath, and of sulphur granules.

Further subdivisions depend upon liquefaction of gelatin, chromogenesis and presence or absence of spores.

There are two decided objections to this system: The use of the term "pseudomonas" to include two very important groups of bacilli, the mono and lophotrichic flagellated rod forms, and the implication that this group contains "pseudomonads"; furthermore, the selection of chromogenesis instead of fermentation of carbohydrate for the second cultural division brings widely divergent species in the same group. As will be seen later on, groups should be of approximately equal size, and as the ratio of chromogenic to non-chromogenic forms is small, this constitutes another objection to the use of chromogenesis in this connection.

In the classification of water bacteria, Fuller and Johnson introduce three very important features:

a. An homogeneous system; variations in the *same* characters are relied upon throughout for division into groups.

b. These characters are of high percentage constancy in actual practice.

c. A graphical method of tabulation of the characters of bacteria is introduced; by this scheme comparisons of bacterial descriptions are much facilitated.

Morphology, the logical basis of any classification, is practically omitted, however, and the descriptions of bacteria are qualitative only; such descriptions have merely a superficial value.

Fischer introduced a purely morphological classification, based upon the arrangement of cells, both with respect to the planes of fission and to branching; flagellation, including the position and arrangement of flagella; spore formation including the position of the spore in the cell, and the form of the spore bearing cell.

It will be noted that no reference is made to the physiology of the organism—cultural and biochemical features are entirely omitted.

B. THE BASIS FOR A CLASSIFICATION.

For a classification to be of value, the following points should be included:

1. It should be homogeneous; the same set of characteristics should be relied upon throughout for division into groups.
2. Morphology should be the basis for such a classification.
3. Any further subdivisions should be dependent upon cultural or biochemical features that will have a high percentage constancy in actual practice.

4. The resulting groups should be of approximately equal size and value; distinction should be made between characters of general classificatory value, and those of confirmatory value only.

5. All characteristics of bacteria relied upon for classification should be expressed in terms that admit of one and only one interpretation.

Since the strucure of bacteria is very simple, only a few morphological characters are available for classification; these are arrangement of the cells with reference to the planes of fission, position and arrangement of flagella, spore formation, spore germination and in certain groups, the flexuosity of the cell.

Of the above characters, the arrangement of the cells with reference to the planes of fission, the presence and arrangement of flagella, and the flexuosity of the cell are the only ones in general use at present.

The flagellation has up to the present time been regarded as of doubtful value by many bacteriologists; this is due largely to defects in technique.

The writer has devised a method (4) for obviating the most serious difficulty in flagella staining, the necessity for repeated manipulation before a sufficient dilution upon the cover glass is attained. This consists essentially in suspending the bacteria from a proper culture in sterile distilled water, and incubating at the optimum temperature of the organism for a short period. By so doing, all foreign matter is removed by sedimentation, and the bacteria alone are transferred to the cover glass. The films are fixed either after evaporation upon the water bath or evaporation at the optimum temperature of the species. By this method, results may be obtained of sufficient constancy to warrant the use of flagellation as a basis for sub-groups under morphology.

Since, as pointed out above, the requisite number of groups cannot be obtained by a purely morphological classification, certain cultural and biochemical features capable of a high efficiency must be employed. This is particularly difficult in the bacteria because of the great confusion surrounding the question of bacterial species.

The state of bacterial knowledge is such at the present time that any attempt to define a species is entirely out of the question. All that can be hoped for is to define certain types, which will be the ultimate limits to which a classification can hope to go. These types will be so general that all forms having important characters in common will be included in one group, and so specific that all forms differing from the group in any important character will be excluded.

This is accomplished in the following manner: The morphological groups, *i. e.*, those groups resulting from purely morphological differences, are further subdivided in the following order; liquefaction or non-liquefaction of gelatin, fermentation (including the production of acid) of dextrose containing solutions, and production of pigment upon slant agar.

Such a system fulfills all the conditions outlined above.

C. DETAILED DESCRIPTION OF THE PROPOSED SYSTEM.

Morphology.

10. Coccaceæ. Cells spherical in the free state, not elongated in any direction before division.

11. Streptococcus, Billroth; planes of fission parallel, giving rise to chains.

12. Micrococcus, Cohn; planes of fission without definite sequence.

13. Sarcina, Goodsir; three planes of fission at right angles to each other.

14. Planococcus, Migula; motile, micrococcus.

15. Planosarcina, Migula; motile sarcina.

20. Bacteriaceæ. Cells elongated, cylindrical; longer diameter always greater than shorter diameter, cells elongated before division.

21. Bacterium, Ehrenberg; non-motile.

22. Bacillus, Cohn; flagellation peritrichic.

23. Bactrillius, Kendall; flagellation mono-trichic (one polar flagellum).

24. Bactrinius, Kendall; flagellation lophotrichic (tuft of polar flagella).

25. [1]Bacterius, Kendall; motile, flagellation unknown.

26. Mycobacterium, Lehmann-Neumann; non-motile, branching form.

30. Spirillaceæ. Cells cylindrical, spirally twisted.

31. Spirosoma, Migula; cells rigid, without flagella.

32. Microspira, Schroeter; cells rigid, with one, rarely two polar flagella.

33. Spirillum, Ehrenberg; cells rigid, lophotrichic (tuft of polar flagella).

34. Spirochæte, Ehrenberg; cells flexous, flagellation unknown.

40. Chlamydobacteriaceæ. Cells enclosed in a sheath.

41. Streptothrix, Cohn; cell division always in one plane.

42. Phragmidothrix, Engler; cell division in three planes, very delicate sheath.

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[1] This group will disappear as knowledge of the flagellation becomes more complete.

NAME.	INVESTIGATOR.	TYPE NUMBER.	BIOLOGY.																																																					REMARKS.
			MORPHOLOGY.								PHYSIOLOGY.																																																
											AGAR.							GEL.			BROTH.			SERUM.			POTATO.						GEL. COLONY.						AGAR COLONY.				MILK.				FERMENTATION					NITRATE.							
			Diameter + 1 micron	Spores. Form in Rod.	Germination.	Capsule.	Stain by Gram.	Grows at 20°.	Grows at 37°.	Fac Anaerobe	Form of Growth.	Elevation of Growth.	Lustre.	Chromogenesis	Topography of Growth.	Optical Characters.	Viscosity.	Liquefaction- Form.	Line Growth.	Surface Growth.	Pellicle.	Turbidity.	Sediment.	Liquefaction.	Form of Growth	Chromogenesis.	Form of Growth	Elevation of Growth.	Lustre.	Chromogenesis.	Potato Discolored.	Gas.	Form of Colony.	Elevation of Colony.	Edge of Colony	Internal Structure.	Chromogenesis	Optical Characters.	Form of Colony.	Elevation of Colony.	Edge of Colony.	Internal Structure	Coagulation.	Liquefaction	Gas	Reaction	Gas in Dextrose.	Gas in Lactose.	Gas in Saccharose.	Growth in Closed Arm	Reaction in Dextrose.	Nitrate.	Ammonia.	Nitrogen	Indol.	Hydrogen Sulphide.	Phenol.	Pathogenesis- Human.	
Str pyogenes	Rosenbach	11.31	+	—	—	—	+	+	+	+	2	3	1	11				—	1	3	—	10	1	—	3	11	1	3	2	11	—	—	1	3	3	5	51		1	3	1	5	3	—	—	+	—	—	—		+				—	+		+	
Mic lanceolatus	Frankel	12.31	—	—		+	+	+	+	+	1	1	1	11				—	2	—	—	11	2	—	2	11	—	—	—	—	—	—	1	3	1	3	51	23	1	3	1	3	3	—	—	+	—	—	—		+				—	—		+	Pneumococcus
Mic. pyogenes variety albus	Rosenbach	12.61	—	—	—	—	+	+	+	+	1	3	1	10				2	—	—	2	21	1				1	1	1	10	+	—	1	1	1	5	10		1	7	1	7	3	—	—	+	·	—	—		+				+	+		+	Staph. pyogenes albus
Mic. zymogenes	MacCallum Hastings	12.61		—	—	—	+	+	+	+	9	1	1	11				2	—	—	—	—	1				9	1	1	11	—	—							1	1	1	5	3	3	—	+		—	—		+				—			+	
Mic pyogenes variety aureus	Rosenbach	12.64	+	—	—	—	+	+	+	+	9	1	1	42				2	—	+	2	20	+	—	3	42	1	2	1	42	+	·	1	1	2	5	51	20	1	3	1	5	3	—	—	+	—	—	—		+				+			+	Staph pyogenes aureus
Mic. gonorrhoeæ	Bumm	12.00	·	—	—	—	—	Does not grow upon ordinary media.																																																				Gonococcus
Bact. rhinoscleromatis	v. Fritsch	21.31	—	—	—	+	±	+	+	+	1	3	1	51				—	2	4	—	11	2	—	1	11	1	3	1	54	—	±	1	4	1	5	51		1	3	1	5	—	—	—	±	+	—	—		+				—			+	
Bact aerogenes	Escherich	21.21	+	—	—	+	—	+	+	+	1	4	1	13		31	+	—	2	3	2	21	3	—	—	11	9	3	1	51	+	+	1	3	1	5	13	24	1	3	1	5	3		+	+	+	+	+	+	+	+			—	+		—	B. lactis aerogenes
Bact. capsulatum	Pfeiffer	21.21		—	—	+	+	+	+	+	9	3	1	10			+	—	1	1	—	11	1	–			9	3	1	51	+	+	1	1	1		13		1	2	1	5	3	—	—	+									+			+	B. mucosus capsulatus, Pfeiffer
Bact. capsulatum	Friedlander	21.21		—		+	—	+	+	+	9	2	1	15			+	—	3	3	2	11	3	—	2	3	9	1	1	51	+	+	1	1	1		10		1	3	1	5	3	—	—	+									+	+		+	B. mucosus capsulatus, Friedlander
Bact. anthracis	Koch	21.41	+	1	2	+	+	+	+	+	5	1	1	11		31	—	2	7	—	4	—	1				1	5	2	53	+	—	1	3	6		11		1	3	6		4	3	·	—	—	—	—	—	+				—	+		+	
B Zenkeri	Hauser	22.11	—	—	—	—	+	+	+	+	9	2	1	10		11	+	—	5	—	—	11	4				9	*	—	—	-	+	7	2			10		7	2			—	—		±	—	—	—						—			—	*Growth invisible B proteus Zenkeri
B. typhosus	Eberth	22.31	—	—	—	—	—	+	+	+	1	1	1	11		22	—	—	2	1	—	11	4	—	2	11	1	*	—	—	—	—	1	1	2	9	51	22	1	3	1	5	—	—	—	+	—	—	—		+	+			—	+		+	*Growth usually invisible
B. coli	Escherich	22.21	—	—	—	—	—	+	+	+	1	4	1	11		22	—	—	2	5	—	21	4	—	2	11	9	3	1	24	—	+	1	7	1	9	51	22	1	3	2	5	3	—	—	+	?	?	?	+	+	+	+		+	+		—	
B. subtilis	Ehrenberg	22.41	+	1	2	—	+	+	+	±	9	3	7	11	7	31	—	2	—	1	7	21	1	+	9	11	1	3	2	51	+	—	1	1		6	11		1	3		6	4	3	—	*±	—	—	—	—	—	—	—	—	—	—	—	—	*Terminal alkalinity
B. vulgaris	Hauser	22.51	—	—	—	—	—	+	+	+	9	7	1	20		*	+	2	2	—	—	21	3				9	1	1	51	+	-	1	4	7		11		1	4	1	9	+	5	—	+	?	—	—	+	+	+	+		+	+	+	—	*Medium darkened B. proteus vulgaris
B. prodigiosus	Ehrenberg	22.53	—	—	—	—	+	+	+	+	1	1	1	33			—	2		—	2	21	1	+	1	31	1	2	1	33	+	—	1	3	1	5	34		1	1	1	5	3	2	—	+	+	—	·	+	+	+	+		+	+		—	
B. janthinus	Zopf	23.67	—	—	—	—	±	+	+	+	1	3	1	73			—	2	—	—	6	11	4				9	1	1	73	+	—	1	7	1	5	71		1	3	1		1	—	—	+	—	—	—		+	+						—	
B. pyocyaneus	Gessard	23.68	—	—	—	—	+	+	+	+	1	6	1	80	4			2	—	—	2	21	4	+	1	80	9	5	1	22	+	—	1	—	1	5	11		1	4	1	6	5	5	—	—		—	—		+	+	+	—	·	—			
B. mirabilis	Hauser	25.51	—	—	—	—	—	+	+	+	9	1	1	20		*		1	2	—	—	21	3				9	1	1	51	+		1	—	7	5	10						5	1	—	-	+	—	—	+	+				+	+			*Medium darkened B. proteus mirabilis
B. paracolon		25.21	—	—	—	—	—	+	+	+	5	6	1	51		24	—	—	3	—	—	11	4	—			1	3	1	22	+	—	1	2	1	5	10	22					—	—	—	*±	+	—	—	+	+	·			—				*Terminal alkalinity
B. enteritidis	Gartner	25.21		—	—	—	—	+	+	+	9	6	1	11				—	1	3	2	11		—	2	11	1	3	1	54	+	—	1	3	7	5	11	22	1	4	1	3	—	—	—	*±	+	—	—	+	+				—				*Terminal alkalinity
B Salmoni	Trevisan	25.21		—	—	—	—	+	+	+	1	1	1	11				—	3	—	—	11	4	—			1	3	1	14	+	—	1			5	51	23					—	—		*±	+	—	·	+	+	—			—	+		?	*Terminal alkalinity B. choleræ suis
B icteroides	Sanarelli	25.21	+	—	—	—	—	+	+	+	1	6	1	51				—	3	—	—	11	4	—			1	3	1	20	+	—	1	3	3	5	11						+	—	—	*±	+	—	—	+	+	—			—			?	*Transient acidity
B. dysentericus	Shiga	25.11	—	—	—	—	—	+	+	+	1	3	1	11		22		—	2		—	11	4				1	1	2	24	+	—	1	3	1	5	51	22	1	3	2	5	—	—	—	*±	—	—	—		—				—			+	*Terminal acidity
Mycobact diphtheriæ	Klebs Loffler	26.31	—	—	—	—	+	+	+	+	3	4	1	11	4	24	—	—	2	1	2	12	1	—	3	11	9	*—	—	-	—	—	1	4	1	5	11		1	1	7	5	—	—	—	†±	—	—	—		+				+	+		+	*Growth invisible †Terminal alkalinity
Mycobact pseudodiphtheriæ	Kruse	26.11	—	—	—	—	+	+	+	+	1	3	1	13		31	—	—	1		—	11	*4	-	1	13	1	5	2	32	+	—	1	3	1	5	51		1	4	1	5	—	—	—	†+	—	—	—						+	+			*Reaction alkaline †Terminal alkalinity
Micros. comma	Koch	32.61	—	—	—	—	—	+	+	+	1	3	1	11				4	1	—	6	21	4	+	1	11	1	1	1	23	—	—	1	—	1	5	51		1	3	1	5	+	—	—	+	—	—	—		+				+	+		+	Spirillum of Asiatic cholera
Micros. Finkleri	Finckler Prior	32.41	—	—	—	—		+	+	+	1	3	1	51				2	1	—	—	21	4	+	1	11	1	1	1	52	—	—	1	—	1	5	11		1	1	1	2	3	3	—	+	—	—							—	+		+	
Micros. tyrogenum	Denecke	32.41	—	—	—	—	—	+	+	+	1	3	1	11		13		2	—	—	2	21	4	+	1	11	—	—	—	—	—	—	1	—	1	5	11		1	1	1	2	—	—	—	+	—	—	—		—	—	—	-	-				
Micros. Metschnikovi	Gamaleia	32.41	—	—	—	—	—	+	+	+	9	3	1	11				2	—	—	3	21	4	+	1	11	1	3	1	21	+	—	1	—	2	5	11						—	—	—	+	—	—	—		-				+	+		+	

f

f

f

f

d

43. Crenothrix, Cohn; cell division in three planes, sheath clearly visible.

44. Cladothrix, Cohn; cells more or less branched.

50. Beggiatoaceæ, cells without a sheath, motility by means of an undulating membrane.

51. Beggiatoa, Trevisan; cells contain sulphur granules.

Cultural and Biochemical Features.

Liquefaction of Gelatin.	Fermentation of Dextrose Gas Production.	Acid Production.
1. negative.	negative.	negative.
2. negative.	positive.	positive.
3. negative.	negative.	positive.
4. positive.	negative.	negative.
5. positive.	positive.	positive.
6. positive.	negative.	positive.
7. *unknown.	negative.	negative.
8. *unknown.	positive.	positive.
9. *unknown.	negative.	positive.

Chromogenesis.

This is the last of the characteristics selected as criteria for division into groups or types.

Chromogenesis is, as usually recorded, subject to considerable variation, but if the determination is made upon slant agar, the results are, under standard conditions, relatively constant.

The principal objection to chromogenesis is the fact that observers do not use the same system in recording observations upon chromogenesis.

The writer, after trying many suggested systems, has made use of the following scheme:

Practically all of the bacterial pigments met with in bacterial research are white, brown, red, orange, yellow, green, blue and violet, or some combination of these, usually two.

Under each spectral color, then, the arrangement is as follows:

1. Spectral color, *e. g.* red.
2. Mixture of spectral color with white, = reddish.
3. Light red, or pink.
4. Dark red, or scarlet.
5. Mixture of the spectral color with brown, = reddish-brown.

6-9. Mixtures of the color under consideration (in this case red) with any other spectral color.

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* These groups contain the thermophilic bacteria, those that do not grow upon gelatin, and those bacteria whose descriptions are incomplete in this respect. As our knowledge of the latter form becomes more complete, they will disappear, and only the thermophilic bacteria, and those forms that do not grow upon gelatin will remain.

The colors are, as mentioned above, arranged in the following order: 1, white; 2, brown; 3, red; 4, orange; 5, yellow; 6, green; 7, blue-violet.

For a detailed table of the different combinations of color, see the paragraph upon chromogenesis in the last section of this paper, upon the method of graphically tabulating the characters of bacteria.

Beginning with morphology, and continuing through chromogenesis, it will be noticed that there are digits placed before each characteristic. They are arranged so that there are two digits before each morphological characteristic, one before the cultural characteristics (liquefaction of gelatin and fermentation of carbohydrate), and one before each of the primary divisions in chromogenesis (white, red, etc.).

It should be remembered in this connection that the above characteristics are the criteria for division into groups, and that the order of division was: Morphology, liquefaction of gelatin and fermentation of carbohydrate and chromogenesis. If, then the digits are arranged in the above order, we would have a graphical instead of a written, statement of morphology, cultural characteristics and chromogenesis. An example will make this clear.

25.34. [1]The two first digits indicate the morphology; under 25 in the table we find bacterius, motile bacillus, flagellation unknown. The third digit indicates liquefaction of gelatin and fermentation of carbohydrate. In the table we find that there is no liquefaction of gelatin, no gas produced, but acid is produced in carbohydrate solution; the fourth digit indicates the *predominating color* upon slant agar. This is in the example given, orange.

To recapitulate: 25.34 is the significant number—the "type number" of a group having the following characteristics: Motile bacilli, flagellation unknown; do not liquefy gelatin; ferment carbohydrate (dextrose) with the production of acid, no gas produced; predominating color on agar slant, yellow.

The writer believes that this classification fulfills all the requirements of a logical system; the classification is homogeneous, the groups are based upon characteristics of high percentage constancy, the groups are of approximately equal size, and by the use of the type numbers, reference to any group is greatly facilitated.

Furthermore, these groups are in reality the *types*, including all the factors that are necessary for division of bacteria into definite families; it will be found that all bacteria having the same type number will agree in all their general reactions, and will be sharply separated from all bacteria having different type numbers.

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[1] It will be noted that there is a period between the first two digits and the last two digits. This is the suggestion of Mr. George C. Whipple that the morphology be separated from the cultural features.

These types, then, are the units around which the species, subspecies and varieties will group themselves. Bacteriologists will all agree that while the question of species is at the present time in confusion, the advancement of certain sharply defined types around which all forms having certain well marked characteristics in common may be grouped, will greatly facilitate any work upon this perplexing problem.

D. METHOD OF GRAPHICAL TABULATION OF THE CHARACTERS OF BACTERIA.

The method of tabulation outlined below is based upon the following considerations:

1. All morphological, cultural and biochemical features of general diagnostic value are included, and a satisfactory description of any bacterial species will be attained when the description is complete upon the given media.

2. For recording the reactions upon the given media, terms admitting of one and only one interpretation are employed.

3. The terms specifically defined below, are chosen with reference to two very important features:

a. So general that all probable reactions or growth-forms upon a given medium will be included.

b. So specific that any reaction or growth-form will be accurately represented, making allowance for the variations that occur with the same culture under varying conditions. In other words, the strength of a chain is the strength of its weakest link; it is useless to make distinctions in reaction of greater precision or definiteness than those which will cover approximately the limits of variation in the same culture under standard conditions.

4. In making comparisons of any two descriptions, identity of symbols is much more apparent than identity of written descriptions.

For this reason, the writer has made use of a decimal system of tabulation; to each of the standard terms indicating a possible reaction of a bacterium upon a given medium, a number is assigned; if those terms include the possible reactions of bacteria upon all the media, we have the possibility of representing graphically or symbolically the reactions of all bacteria upon all media. The numbers represent in graphical form the characteristics of bacteria.

Chester (5) has published a set of terms covering the possible reactions of bacteria upon the different media, and these terms admit of only one interpretation. Furthermore, they are in general use among bacteriologists at the present time.

These terms have been slightly modified for increased definiteness and completeness and in their modified form have been used in this scheme for graphical tabulation.

Detailed descriptions of growth forms are made upon gelatin and agar plates, agar, serum and potato slants, gelatin stabs, milk and bouillon.

Other biochemical reactions, nitrate, indol, hydrogen sulphide, phenol, etc., are indicated as positive (+) and negative (—).

The general arrangement will be apparent by referring to the table appended, in which certain bacteria are tabulated according to the method outlined above.

CONCLUSION.

The advantages claimed for this system are:

1. An homogeneous system including sharply defined groups to which one may refer with a minimal expenditure of time and trouble.
2. Uniformity in choice of terms.
3. Specificity of terms.
4. Completeness of descriptions.
5. Facility with which descriptions may be compared.

It may be argued that this system is complicated, *but it must be remembered that the facts to be explained are complicated.*

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SCHEME FOR THE GRAPHICAL TABULATION OF THE CHARACTERISTICS OF BACTERIA.

MORPHOLOGY.

Form, flagellation (including motility) as described above.

Spores.

Form and position of spore in the cell.

1. Form of cell unchanged, spore equatorial.
2. Form of cell unchanged, spore polar.

3. Cell swollen, spore equatorial.
4. Cell swollen, spore polar.

Method of germination of the spore.

1. Germination equatorial.
2. Germination polar.
3. Germination by absorption.

CULTURAL FEATURES.

Bouillon; pellicle, turbidity and sediment.

Pellicle.

1. O pellicle, ring formed.
2. Pellicle membranous.
3. Pellicle tenacious (tough).
4. Pellicle flocculent.
5. Pellicle granular.
6. Pellicle wrinkled, thin.
7. Pellicle wrinkled, thick.

Sediment.

1. Sediment flocculent.
2. Sediment granular.
3. Sediment viscous.
4. Sediment compact—amorphous.

Turbidity.

10. Turbidity slight, clears up on standing.
11. Turbidity slight, non-characteristic.
12. Turbidity slight, flocculent.
13. Turbidity slight, granular.
14. Turbidity slight, viscous.
20. Turbidity decided, clears up on standing.
21. Turbidity decided, non-characteristic.
22. Turbidity decided, flocculent.
23. Turbidity decided, granular.
24. Turbidity decided, viscous.

SLANT CULTURES (AGAR, SERUM, POTATO, ETC.)

Form of Growth.

1. Filiform; uniform growth, without special characters.
2. Nodose; consisting of rather large, closely aggregated colonies.
3. Beaded; consisting of small, closely placed or disjointed colonies.
4. Echinate; beset with acicular extensions.
5. Villous; beset with short, undivided hair-like extensions.

6. Plumose; a delicate, feathery growth.
7. Arborescent, tree-like, with branched hair-like extensions.
8. Grows only in the condensation water (on agar).
9. Spreading; grows over the whole slanted surface.

Elevation of Growth.

1. Flat; thin, spreading over the surface.
2. Effused; more delicate than the preceding, spreading.
3. Raised; growth thick, with abrupt terraced edges.
4. Convex; surface the segment of a circle.
5. Capitate; more convex than the preceding, hemispherical.
6. Umbilicate; depressed in the middle like a navel.
7. Umbonate; having a boss or elevation in the middle.

Topography of Surface Growth.

1. Smooth; surface even without any of the following distinctive characters.
2. Alveolate; marked by depressions separated by thin walls, like a honeycomb.
3. Punctuate; dotted with minute punctures, like pin pricks.
4. Bullate; covered with blister-like prominences.
5. Squamose; covered with scales.
6. Papillate; beset with papillæ, or mamma-like processes.
7. Rugose; short, irregular folds.
8. Contoured; an irregular, but smoothly undulating surface like the surface of a relief map.
9. Rimmose; abounding in clefts or cracks.

Optical Characters.

10. Transparent.
11. Vitreous; transparent and colorless.
12. Oleaginous; transparent and yellow, olive-linseed oil colored.
13. Resinous; transparent and brown, varnish or resin colored.
20. Translucent.
21. Paraffinous; translucent and white.
22. Opalescent; translucent, grayish-white by reflected light, smoky-brown by transmitted light.
23. Nacreous; translucent, grayish-white with pearly lustre.
24. Sebaceous; translucent, yellowish or grayish-white, tallowy.
25. Butyrous; translucent and yellow.
26. Ceraceous; translucent and wax colored.
30. Opaque.
31. Cretaceous; opaque and white.
32. Chalky; dull, without lustre.

Chromogenesis.

10. Snow-white.
11. Gray.
12. Cream-white.
13. Porcelain-white.
14. Gray-brown.
20. Brown.
21. Brownish.
22. Brownish-red.
23. Brownish-yellow.
24. Brownish-green.
30. Red.
31. Reddish.
32. Flesh color—pink.
33. Dark red—scarlet.
34. Red-brown.
40. Orange.
41. Orange-white.
42. Orange-yellow.
43. Orange-green.
44. Orange-brown.

Lustre of Growth.

1. Shining.
2. Dull.
50. Yellow.
51. Yellowish.
52. Light yellow; lemon-sulphur.
53. Yellow-green; citron yellow.
54. Yellow-brown.
60. Green.
61. Greenish.
62. Light green.
63. Dark green.
64. Greenish-yellow.
65. Greenish-brown.
70. Blue—violet.
71. Bluish.
72. Light blue.
73. Indigo-blue.

STAB CULTURES (GELATIN OR AGAR).

Non-Liquefying, Line Growth.

1. Filiform; uniform growth, without special characters.
2. Nodose; consisting of closely aggregated colonies (rather large).
3. Beaded; consisting of loosely placed or disjointed colonies.
4. Echinate; beset with acicular extensions.
5. Villous; beset with short, undivided, hair-like extensions.
6. Plumose; a delicate feathery growth.
7. Arborescent; branched, tree-like; with branched, hair-like extensions.

Liquefying, Line Growth, Form of Liquefaction.

1. Crateriform; a saucer-shaped liquefaction.
2. Saccate; shape of an elongated sack, tubular or cylindrical.
3. Infundibuliform; shaped like a funnel, conical.
4. Napiform; shape of a turnip.
5. Stratiform, liquefaction extending to the walls of the tube, and downward horizontally.

Surface Growth, non-liquefying cultures. (Also plate colonies.) See Elevation, slant cultures.

Plate Colonies (Gelatin and Agar).

Form of Colony.

1. Round.
2. Fusiform; elliptical, tapering at each end.
3. Conglomerate; an aggregation of similar colonies.
4. Cochleate; spiral or twisted like a snail shell.
5. Amoeboid; very irregular, streaming.
6. Myceloid; a radiating structure like a mould.
7. Filamentous; an irregular mass of densely woven filaments.
8. Rhizoid; an irregular branched or root-like colony.
9. Rosulate; rosette shaped.

Surface Elevation of Colony. (See Slant Cultures.)

Detailed Characters of Surface. (See Topography of Slant Cultures.)

Internal Structure of Colony. (Microscopic.)

1. Amorphous; without definite structure.
2. Hyaline; clear and colorless.
3. Homogeneous; structure uniform throughout.
4. Areolate; divided into rather irregular, or angular spaces by more or less definite boundaries.
5. Grumose; clotted appearance, particles in clustered grains.

6. Moruloid; having the appearance of a morula, divided into more or less regular spaces.

7. Clouded; having a pale ground with ill-defined patches of a deeper color.

8. Gyrose; marked by wavy lines.

9. Marmorated; showing faint, irregular stripes, or traversed by vein-like markings, as in marble.

Edge of Colony.

1. Entire, without toothing or division.
2. Waved; slightly undulating.
3. Lobed; consisting of lobes.
4. Serrated; notched on the edge like a saw.
5. Auriculate; with ear-like lobes.
6. Ciliate; hair-like extensions, radially placed.
7. Erose; as if grawed, irregularly toothed.

Optical Characters. (See Optical Characters of Slant Cultures.)

MILK CULTURES.

Coagulation.

1. Coagulates only on boiling.
2. Coagulation in the cold, coagulum hard, flocculent.
3. Coagulation in the cold, coagulum hard, compact.
4. Coagulation in the cold, coagulum hard, granular.
5. Coagulation in the cold, coagulum soft, flocculent.
6. Coagulation in the cold, coagulum soft, compact.
7. Coagulation in the cold, coagulum soft, granular.
8. Digestion without a coagulum being formed.

Liquefaction of Casein.

1. Liquefaction slight, whey turbid.
2. Liquefaction slight, whey clear.
3. Liquefaction decided, whey turbid.
4. Liquefaction decided, whey clear.
5. Liquefaction complete, whey turbid.
6. Liquefaction complete, whey clear.

ON THE CLASSIFICATION AND IDENTIFICATION OF BACTERIA WITH A DESCRIPTION OF THE CARD SYSTEM IN USE AT THE LAWRENCE EXPERIMENT STATION FOR RECORDS OF SPECIES.

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Among the various branches of natural science there is probably not one which in comparison to the volume of its data has so slight a basis for classification as has bacteriology. In the early days, this lack of a good systematic classification made progress extremely slow, and even today acts as a drag upon a branch of investigation which is rapidly assuming a more important aspect as its relation to the public health becomes better understood. A classification such as is employed in other branches of natural science, *i. e.*, one based entirely on morphology or external differences, is, for the present at least, almost out of the question, and we are forced to include with our morphological data certain other features of growth or biochemical reaction. A decade ago, if an investigator wished to compare his own work with the descriptions of organisms previously reported, it was necessary for him to go through an almost endless mass of written descriptions, a task which the majority of writers of that time failed to perform, simply recording their own work and leaving for the student of the future the work of classifying and arranging this immense volume of data.

The question of bacterial genera was early taken up by many workers, with the result that nearly as many different systems of classification were proposed. In this connection it is only necessary to mention the early form-genera of Cohn,[1] based on the form and size of the organism, the later system of Cohn[2] in which the method of sporulation was made a basis for the subdivision of the larger form-genera, that of A. Fischer[3] based on the form and method of flagellation, and finally the method of Migula,[4] in which the form, method of subdivision and arrangement of flagella were made the criterion for generic arrangement. Many other workers, as Flügge, Schroter, DeBary, Koch, Hueppe and others have modified these systems in one way or another without adding anything new. The system of Migula seems to be a fortunate combination of the best points of the above, and has come into general acceptance. Chester, in his admirable compilation of the bacterial literature of the past has employed this system, and it seems as though at last we have a fairly permanent set of bacterial genera. Further than this we may not hope to go at present, if we are to confine ourselves strictly to morphological characteristics.

In 1889, Fuller and Johnson[5] proposed a new arrangement of species, based entirely on certain bio-chemical and cultural reactions which they had demonstrated to be constant. While the method of tabulation proposed by the above writers has been quite widely adopted, their method of grouping bacteria has not received the complete acceptance necessary for a permanent system. As the use of their system of recording results in tabular form has become extended, another defect becomes apparent, namely that in each published table of bacteria, the tests used and the arrangement of these tests in tabular form, each varies from the other, so that with the ever increasing number of tables the comparison of a new species with those already described becomes more and more of a task. It is evident to all that some permanent, uniform and convenient system of classification and recording descriptions of bacteria is needed. Such a system must not disturb existing systems more than is necessary, must be based on features which are constant and can be readily duplicated, must be sufficiently elastic to allow for such changes as the rapid advance in the knowledge of bacteriology will require, and above all must reduce and simplify the labor of the comparison of species descriptions.

In reference work and in general business, the card system for records has been found to be the only one which was sufficiently elastic to meet all the requirements, and this usually when arranged according to a definite and prescribed numerical scheme.

In a paper before the Bacteriological Convention in 1895, the late Wyatt Johnston,[6] speaking in a mood truly prophetic, said: "It has occurred to me that all the important characteristics of a given species might be recorded more compactly than at present, if a system were adopted by which the information furnished by the various tests could be represented by means of numbers, each stated in a definite order so as to form a code.

In this the type of growth on different media, staining, fermentation, etc., would be recorded in a very condensed form, capable of being taken in at a glance, or so, and that the comparison of any one species' description with any other would be a rapid and easy instead of a troublesome and tedious affair. This would differ from the existing keys in that every leading characteristic would be given in the case of every species, and thus form a sort of cross index to the species when grouped in a natural manner. * * * Our present position with regard to the grouping of species of water bacteria may be summed up as follows: The descriptions at present recorded * * * lay stress on points of difference to the neglect of points of resemblance. Too much importance is attached to descriptions of conditions which cannot be readily utilized by others and too little attention paid to tests by which definite information (yes or no) is obtained. * * * it is highly important that a more uniform plan should be followed in describing a species if they are to be grouped correctly.

"For purposes of grouping a single, strongly-marked characteristic, peculiar to a few species is of more value than a number of minor details, but the members of any group should not differ unduly, in regard to minor points."

In the decimal system as applied to general literature, the numbers are arbitrarily assigned. For the arrangement of species in groups, by a numerical system, the numbers must necessarily be arbitrarily assigned, but these numbers can be so logically derived that only a minimum of labor would be required of the average working bacteriologist to derive the whole system in case he had forgotten the key.

The attempt at a numerical classification at the Experiment Station arose in an effort to classify existing bacterial literature under the decimal system in common use for library cataloging. The credit for the first practical suggestion along this line should be given to A. I. Kendall, at that time a member of the bacteriological force at the Experiment Station, from whose preliminary scheme the system in use at the present time has been the logical outgrowth. The system of numerical classification has already been described by one of the writers,[7] the extension of this system to the complete record, with the use of cards, is here described for the first time.

In the current paper the writers propose to describe the complete system as it is used at present at the Experiment Station in the belief that it possesses some distinct advantages over any other system which has come to our notice, and that only by a widespread publication and discussion of the subject by practical workers can the good points be retained and the chaff eliminated.

CLASSIFICATION OR ARRANGEMENT OF BACTERIA IN GROUPS.

In the system all of the characteristics of a species are described by number, this number being derived from the combination generally of two or more allied characteristics.

The group is represented by four figures, of which the first two digits signify the morphological genera, the form, method of division, motility, and arrangement of flagella, according to the accepted classification of Migula.

These are derived as follows:

1. *Coccaceae.*
  11. Streptococcus, division in one plane.
  12. Micrococcus, division in two planes.
  13. Sarcina, division in three planes.
  14. Planococcus, motile coccus.
  15. Planosarcina, motile sarcina.

2. *Bacteriaceae.*
  20.† Motile rods, flagella not determined.
  21. Bacterium, non-motile rods.
  22. Pseudomonas, *a*, motile rods, flagella monotrichic.
  23. Pseudomonas, *b*, motile rods, flagella lophotrichic.
  24. Bacillus, motile rods, flagella peritrichic.

3. *Spirillaceae.*
  31. Spirosoma, cells rigid, without flagella.
  32. Microspira, cells rigid, one (rarely 2-3) polar flagellum.
  33. Sprillum, cells rigid, polar flagella tufts.
  34. Spirochæta, cells flexuous.

The morphological genera are sub-divided by the differences in certain well recognized cultural and biochemical features.

This sub-division, forming part of the whole group number, consists of two digits, of which the third group figure indicates the biochemical features of the liquefaction of gelatin and the production of gas or acid in dextrose broth, as suggested by Groups IX to XIII of Fuller and Johnson. The derivation of the nine digits used in this place is as follows:

	Gelatin.	Dextrose Broth.
1	Non-liquefied.	No gas or acid produced.
2	Non-liquefied.	Acid produced, no gas.
3	Non-liquefied.	Gas produced.
4	Liquefied.	No gas or acid produced.
5	Liquefied.	Acid produced, no gas.
6	Liquefied.	Gas produced.
7*	Doubtful.	No gas or acid produced.
8*	Doubtful.	Acid produced, no gas.
9*	Doubtful.	Gas produced.

---

† 20 is provisional group, and should disappear, its members falling into groups 22, 23 or 24.

* In groups 7, 8 and 9, would be included the so-called thermophylic species which do not grow at the temperature at which gelatin is solid.

The digit in the fourth place shows the fluorescence and chromogenesis on agar, the grouping being similar to that in Groups I to VIII of Fuller and Johnson.

These are designated as follows:

1. All fluorescent species, irrespective of their chromogenesis.
2. All red chromogenic species.
3. All orange chromogenic species.
4. All yellow chromogenic species.
5. All blue and violet chromogenic species.
6. All green chromogenic species.
7. All brown chromogenic species.
8. —— —— —— ——
9. All others not included in the above.

By the use of such a system as the above, the arrangement of bacteria in groups becomes at once a simple and an exact matter. We no longer have the old, rather indefinite question, does our species belong to the colon group, or to the hog cholera group, or to some other equally indefinite group? We simply drop our colon bacillus into slot No. 2439, or our sewage streptococcus into No. 1129, and they immediately find themselves in congenial company.

NUMERICAL METHOD OF RECORDING BACTERIAL REACTIONS.

The system of recording reactions by the means of plus and minus signs, used by Fuller and Johnson in their compilation of the bacterial flora of the Ohio river, was a distinct advance over the verbose written descriptions of earlier investigators.

This method of recording, has found a ready acceptance by many writers, and as a result we have a number of tables of bacterial descriptions. These are all open to the serious objection, that while the literature is in a more accessible form than was that before the advent of this method, the use of it necessitates the employment of large folded inserts which are difficult to handle when fastened into bound books, and upon which the wear and tear in routine study is considerable.

The reference to a long line of signs, even with the aid of a straight-edge is difficult, and this difficulty is further complicated by the fact that no two writers have used exactly the same order in the arrangement of data. The writers would recommend in this respect that a standard order for the recording of the essential species characteristics be adopted by some such body of representative bacteriologists as is this Section, which order once adopted, would undoubtedly be followed by a considerable number of investigators of the immediate future, to the consequent benefit of all.

To reduce the size of the tables and render comparisons more easy we may either reduce the number of tests tabulated or we may combine these tests in some manner so that two or more tests may be recorded in the space at present occupied by one.

The reduction of the number of tests is not feasible if we would wish to have anything like complete data. As a means for combining the data without curtailing either its volume or its usefulness, the numerical system here described has proved quite successful.

As a basis for the system we start with the assumption that all of the tests usually applied in species differentiation may be logically divided into groups of three, any one, two or three of which may be positive. Arranging these three tests, A, B, C, in the order of negativity we derive eight numbers, each one representing some combination of the positive and negative values of the three tests, as follows:

	A.	B.	C.	
1	0	0	0	All negative.
2	0	0	+	
3	0	+	0	Two negative, one positive.
4	+	0	0	
5	0	+	+	
6	+	0	+	One negative, two positive.
7	+	+	0	
8	+	+	+	All positive.

A glance at any one of the tabular descriptions of species of the present day will show that there are usually three characteristics recorded on each medium, and an arrangement by media is at once suggested. There are some tests, however, which at first appear to introduce complications. For example, in the arrangement of tests further on, under milk, we have coagulation, acid reaction, and alkaline reaction as values for A, B, C in the table of numbers. It is manifestly impossible for a reaction to be at the same time acid and alkaline.

This, however, does not vitiate the values for the other numbers, the digits 5 and 8 simply becoming non-existant. Under aerobiosis we have a more extreme case, only one of the three values used being possible at one time. In this case all of the digits except 2, 3 and 4 become silent, but as we are concerned only with the live numbers, this tends to simplify, rather than to complicate matters. In the case of pathogenesis and the indol reaction, where we have the positive and negative values for only one function, we would use only the digits 1 and 8 as representing the extreme negative and positive limits.

TABLE SHOWING GROUPING OF TESTS UNDER NUMERICAL SYSTEM.

Column.	Group.	A.	B.	C.	Digits.
1	Morphology	Spores	Capsules	Grams stain	1 to 8 inclusive
2	Liquefaction	Gelatin	Casein	Serum	1 to 8 inclusive
3	Dextrose broth.	Gas	Acid	Growth in closed arm	1 to 8 inclusive
4	Lactose broth	Gas	Acid	Growth in closed arm	1 to 8 inclusive
5	Saccharose broth	Gas	Acid	Growth in closed arm	1 to 8 inclusive
6	Milk	Coagulated	Acid	Alkaline	1, 2, 3, 4, 6, 7 only
7	Nitrates reduced	Nitrites	Ammonia	Nitrogen	1 to 8 inclusive
8	Indol produced				1 and 8 only
9	Nutrient broth	Turbid	Pellicle	Sediment	1 to 8 inclusive
10	Gelatin stab	Crateriform	Infundibuliform*	Stratiform	1, 2, 3, 4 only
11	Agar streak	Luxuriant	Dull or wrinkled	Viscous	1 to 8 inclusive
12	Potato	Visible	Luxuriant	Discolored	1, 2, 4, 6, 7, 8 only
13	Temperature	Grows better at 20° C.	Grows equally well at 20° or 38°	Grows better at 38° C.	1, 2, 3, 4 only
14	Aerobiosis	Obligate aerobe	Facultative	Obligate anaerobe	2, 3, 4 only
15	Pathogenesis				1 and 8 only

* Including napiform and saccate.

NOTE—A practical illustration of this system will be found in a paper on B. coli and allied species, by the authors, published elsewhere in this volume.

Of course it would be possible to have a separate arrangement for such cases as these, or we might combine two or more of them, but this would tend rather to confusion than to simplicity. Throughout the system as arranged, only similar tests have been combined, the application of a number of other combinations having been shown by a trial to lead to confusion.

In the application of the system to the various tests, only such tests have been retained as have given constant results, and which can be duplicated at any time with media of a constant composition and with cultures in the proper condition for study, *i. e.*, after efficient preliminary cultivation. The order of the tests is based on their probable value in species differentiation. This order, with the division of the tests into groups, and the various figures representing the various combinations which would appear in a tabulation of species by this method, is shown in the preceding table.

CARD SYSTEM FOR RECORDS OF BACTERIAL SPECIES.

In the study of any considerable number of species of bacteria, the difficulty in comparing the records of the various cultures with one another, and with the published descriptions of species is immediately apparent. This is especially true if these records are in books. Attemps to use loose sheets of various forms have been tried at different times, but in the past, the rapid change in ideas has usually rendered these forms short lived. The very complete forms prepared by Dr. Cheesman for the Bacteriological Committee are open to the one serious objection of extreme bulkiness, and a comparison of any considerable number of species recorded on them becomes a laborious task.

With the advent of records by plus and minus signs, and by the use of some such form of taxonomy as that used by Chester in his compilation of bacterial species, it becomes possible to condense these descriptions into a comparatively small space. Since many of the features to which earlier writers devoted elaborate descriptions have been proved to be extremely variable, even in duplicate determinations, we have come to confine ourselves more and more to tests which not only yield constant results, but which can be described in a few words or by some sign or readily understood symbol, with the result that a complete description on a form which is easily handled and filed in its proper place becomes a possibility.

The writers have devised a form for use at the Experiment Station, which proves to be so convenient, that it is here described for the benefit of others who wish to break away from the inconveniences of the older methods of keeping species records. The form shown is self-explanatory. Records are made in the columns on the right by plus and minus signs and a few well-known symbols. There is sufficient room in the body of the blank for notes and such descriptive matter as is not recorded in the columns, while additional notes and drawings may be made at the bottom and on the back of the card.

The entries in the spaces across the top are made by the numerical system heretofore described. The form is printed on a good grade of card stock, this being easier to handle and more durable than paper. The size of the card, eight by ten and one-half inches, is the same as the regular letter sheet, and cards may be filed in any of the numerous vertical cabinets which are on the market. The cards are filed in numerical order by the system already described. By this method, similar species come on adjacent cards and it is a simple matter to weed out identical cultures. Comparisons of descriptions are made by laying the cards down so that only the marginal columns are visible, the comparison being either the plus and minus signs or the numerical nomenclature, the numerical grouping rendering the comparison of only a few cards necessary at any one time. We have found it to be very convenient to enter descriptions of other writers on this form, filing these cards with our own descriptions, thus eliminating reading back and forth from one system to another. The reduction of other descriptions to this form is not as laborious as would at first appear, the ruling of the form being so spaced that the copying may be readily done on the typewriter.

The ruled form follows:

Group.	Morph.	Liq.	Dex.	Lact.	Sac.	Milk.	KNO_3.	Indol.	Broth.	Gel.	Agar.	Potato.	Temp.	Aerobe.	Path.		Culture.
NAME.						INVESTIGATOR.					SOURCE.				ISOLATED.		

	Morphology.	Spores	Length	Diameter	Form
		Capsules	Gram's	Flagella	Motile
	Gelatin Stab.			Form	Liq.
	Gelatin Plate.			Form	Liq.
Dextrose.	Fermentation Tube.		C. A.	Acid	Gas
Lactose.			C. A.	Acid	Gas
Saccharose.			C. A.	Acid	Gas
	Agar Streak.	Lux.	Viscous	Wrinkled	Dull
				Chrom.	Fluor.
	Agar Plate.				
	Broth.		Sediment	Pellicle	Turbid
	Milk.		Reaction	Pept.	Coag.
	Nitrates.		N.	NH_3	HNO_2
	Indol.				
	Potato.		Discolor	Lux.	Visible
	Serum.				Liq.
	Pathogenesis.				
	Litmus Lactose Agar.			T. B.	Red
	Temperature.			38°C.	20°C.
	Odor.				

(Form used for bacterial descriptions at the Lawrence Experiment Station.)
(Reduced.)

CONCLUSION.

In order to ensure the permanency of descriptions of bacterial species, the writers believe that only such tests should be employed as give constant results and can be duplicated at any time, and that before being submitted to these tests all cultures should be put through an efficient course of revivification.

The numerical method of classification possesses the advantage that while arbitrary, it is logical and definite, no further labor being required, after the group number has been carefully worked out, in order to place the description with others which are similar. This will be of assistance in classifying bacterial literature according to the well-known Dewey system, the group number being used as a portion of the decimal under this system. The system is based on existing conditions, which are thoroughly established, and is at the same time so elastic that any future discoveries may be incorporated without in any way vitiating the value of the system as a whole.

The system of recording bacterial description in the past has been so bulky as to render these descriptions difficult of access. The use of plus and minus signs was an improvement over the older methods of recording bacterial reactions, but is open to the objection that no two writers have used the same order in the arrangement of data, and that its employment necessitates the use of very large tables which are difficult to handle for reference. The numerical system, establishes a definite order for recording tests, and cuts down the size of the table to about one-third without impairing its usefulness. The numerical system also opens up a large possibility for the elimination of the confusion which has occurred in the past in the nomenclature of bacterial species, since by its use, the similarity or identity of two published descriptions becomes at once apparent with the result that the name of a species is readily established by the law of priority.

The most serious objection to the numerical system as here presented, lies in the fact that an incomplete record will apparently throw the whole system out of operation. This is overcome by recording all of the possible numbers which might occur if the record were complete. While this is somewhat of a drawback to the use of the system, it does not in any way interfere with its efficiency, and, moreover, would have one very good effect on future work in that it would tend to make observers more exact and more complete in recording the characteristics of a species, to the considerable advantage of other workers who are interested in the description from a somewhat different standpoint.

The value of the card system will be apparent to all who have had occasion to trace out a number of cultures, eliminating duplicates, and trying to establish identity with previously described species.

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AN EXAMINATION INTO THE VALUE OF CERTAIN ANTISEPTICS USED FOR THE PRESERVATION OF ANTITOXIC AND OTHER IMMUNE SERUMS.

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The method of conducting the research was as follows:

Different proportions of the antiseptics were added to freshly-drawn blood serum, previously allowed to stand for a number of days, until its natural germicidal properties had disappeared.

(1) To 10 c. c. of each preparation, a measured quantity of a fresh bouillon culture of the bacillus prodigiosus was added. This was allowed to stand a given length of time, after which 1 c. c. of the contaminated serum was taken from each tube, mixed with 10 c. c. of agar-agar, poured into a Petri dish, stood aside for two days, and the number of colonies counted.

(2) Upon another occasion, by the same general method, 1 c. c. of a dilution of the 24-hour-old culture of the colon bacillus was similarly added into each preparation and 1 c. c. transferred to a tube of melted agar-agar, poured into a Petri dish and allowed to stand for the given length of time, and the colonies counted.

(3) In the same way, upon another occasion, 1 c. c. of a culture of the bacillus subtilis was added to each, and similarly plated, and the colonies counted.

Experiments were made with chloretone, formaldehyd, trikresol and phenol, and resulted in demonstrating that formaldehyd is the most efficient protective, phenol next. Formaldehyd is most efficient in a strength of 1:1,000, and in this strength greatly outweighs any of the other antiseptics used for the experiments. Smaller proportions of the formaldehyd fail to protect the serum, and there seems to be a very marked difference in this regard between different quantities of the formaldehyd. Thus, while 1:1,000 formaldehyd was sufficient to destroy all of the bacilli prodigiosi, bacilli coli and bacilli subtilis added, 1:2,000 entirely failed so to do, the serum becoming hopelessly contaminated by them.

A synopsis of the experiments is as follows:

(1) The chloretone succeeded in destroying the bacilli prodigiosi, and colon bacilli, but even after a week's action, entirely failed to destroy the bacilli subtilis. Chloretone, therefore, is of doubtful value as a protective.

(2) Formaldehyd in a proportion of 1:5,000 destroyed all of the bacilli prodigiosi and all of the bacillus coli cultures, but failed to destroy bacillus subtilis. 1:1,000 strength of formaldehyd at the end of seven days, had succeeded in destroying all of the spore forming bacilli prodigiosi added to the serum. None of the serums to which formaldehyd was added were in any way changed in appearance or brilliancy, and none became viscid.

(3) Trikresol was used in 0.4, 0.5, 0.8 and 1 per cent., the usual flocculent precipitate entirely spoiling the appearance of the preparation. All of these strengths were capable of destroying bacillus prodigiosus with the exception of 0.4 per cent. The growth of the bacillus coli was also restrained. It required 1 per cent. of trikresol ultimately to destroy the colon bacillus. Trikresol even in a proportion of 1 per cent., failed to kill the bacillus subtilis in seven days, though it might have done so in longer time, as it was found that the number of colonies progressively diminished and diminished most rapidly in the serum containing the greater proportion of antiseptic. However, 1 per cent. of trikresol failed to kill all of the bacilli subtilis in seven days.

(4) Phenol was investigated in proportions of 0.4, 0.5, 0.8, and 1 per cent., all of these succeeded in killing the bacillus prodigiosus. They all exerted a restraining action of the bacillus coli, though after several days a few colon bacilli grew colonies upon the dishes even with the 1 per cent. dilution. Phenol exerted a somewhat greater destructive power upon the bacillus subtilis than trikresol had done.

The only antiseptic that destroyed all the bacilli introduced into the cultures was formaldehyd in a proportion of 1:1,000.

Recommendation. Upon the strength of these investigations, other things being equal, formaldehyd is to be preferred to phenol, and phenol to be preferred to trikresol as a preservative for the serum. Formaldehyd, because in very small amounts it not only restrains the growth of the bacteria, but destroys them, and because it produces no alteration in the quality of the serum, so that its filtration becomes unnecessary. Formaldehyd, 1:1,000, does not kill molds.

Phenol restrains the growth of bacteria and destroys them somewhat better then trikresol, producing less precipitation than the trikresol and not changing the color of the serum. In a proportion of 1:1,000 formaldehyd did not increase the viscidity of the serum to which it was added. Unless the pain occasioned by the injection of formaldehyd into the tissues or the poisonous influence of the phenol should be held to outweigh the advantages of the trikresol, I certainly recommend that we abandon its use in the future.

NOTES ON FORMALDEHYDE.

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A re-examination of disinfection by formaldehyde gas was undertaken in 1902 at the instance of the Boston Board of Health.

It was determined to conduct the experiments for each of the ordinary pathogenic bacteria, one at a time. Many interruptions occurred, so that the present article is a report of progress only, and much if not all of the work so far done has been done before. It was found impossible to review the literature adequately or to give credit to those who suggested directly or indirectly the lines of work followed. Acknowledgments must, however, be made to the article of Dudley and McDonnell[1] for some very useful suggestions.

Attention was confined largely to one bacterium (*B. diphtheriae*) and to one method of exposure, the bacilli dried upon glass rods after the method of one of the writers. (Hill.) The organisms in this condition are as resistant as in any other by such condition—according to Ravenel and others, more so.

The methods used were devised with the object of so handling the gas that the test objects might be dipped into and withdrawn from it with the same ease and accuracy attained in testing liquid solutions. These methods followed those of Dudley and McDonnell in general principle.

A tight wooden box was constructed containing 100 cubic feet (dimensions, 6 2-3 feet x 5 feet x 3 feet). Both sides and both ends of the box were perforated at regular intervals with round, ¾-inch holes, 139 in all, designed to take ¾-inch solid corks.

Every hole in the box being closed with ordinary solid corks, the box could be filled with exact quantities of gas introduced through a cork hole from a generator or from an atomizer, or evaporated from a copper pan which was so fitted into the bottom of the box as to allow the application of heat directly to it from the ouside. This pan could be filled with water, formalin or paraform before the experiment, or, after closing the box, water or formalin could be introduced through a long stemmed funnel reaching the pan through a convenient cork hole. An electric fan in the box, controlled from the outside, could be used when desired for stirring up and distributing the contents equably.

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[1] American Engines and Railroad Journal, June, 1902.

The glass rods used as test objects were about 7 inches long and 3-16 inches in diameter. One end of each rod was inserted into a central perforation in a ¾-inch cork, in such a manner that the rod passed through the stopper from the narrow to the broad end, projecting about ⅛ of on inch beyond the broad end. The other end of the rod thus projected in line with the diameter of the cork and in the direction of the taper. The rods, each in a clean ¾-inch test tube closed by the cork on the rod were sterilized by dry heat. This free end of each rod was then inoculated with the bacteria to be tested.

The test object rods were smeared with bacteria by rolling the free ends in the emulsion obtained by mixing a 24-hour growth at 37° C. on an agar slant with the condensation water of the same tube. The rods were set to dry in a box similar in construction to the formaldehyde box, but much smaller and having cork holes in two sides and the top sufficient for 450 rods at once. One end of this box was glass, the other cloth, admitting diffused sunlight and permitting slow diffusion of air.

The exposure of a test object, when the test box was filled with gas, was accomplished by removing quickly one of the solid corks and instantly slipping the rod, inoculated end first, into the hole, the cork on its other end closing the hole and at the same time keeping the rod in position with its inoculated end projecting into the box. To remove the test object, this process was simply reversed. No perceptible amount of formalin was lost in the process, each hole being open for about three seconds only. Twenty rods could be inserted or removed by one man in about three minutes.

After the withdrawal of a rod from exposure in the formaldehyde box, it was inserted, inoculated end first, into an agar slant tube. The inoculated end was then rubbed thoroughly on the agar to remove the bacilli from the rod and, further to encourage growth, the rod was left in the tube, its end impinging on the agar or lying in the condensation water. In this condition, the cork on the upper end of the rod sealed the tube as a cotton plug might do. In taking out a series of test objects, each, to save time, was simply dropped into its own agar tube, the rubbing on the agar, etc., being done afterwards. Thereafter each tube was incubated at 37° C. and examined every day until growth occurred or a satisfactory time had elapsed without growth. Each day the tubes found negative on that day were tipped up to run the condensation water over the agar, so that every chance for development might be given.

The cultures of *B. diphtheriae* used came from about forty different patients. They were kept on agar at 37° C. and were transferred from day to day so that fresh tubes 24 hours old were always on hand. From 10 to 32 of these cultures were used in each experiment, each set of rods representing a corresponding set of cultures, the design being to determine the variations in resistance of different cultures. No such variations were discovered, however. By numbering the cultures, the holes in the small drying box and the holes in the large formaldehye box, it was easy to keep records showing the exact position of every rod in each experiment and of the culture from which it came. About a thousand rods have so far been thus exposed.

Using these methods, the test objects might be set in place before or after the release of the gas or might be exchanged for new test objects during the course of the experiment. More gas might be added or the humidity increased at any time. The action of a given body of gas at a low humidity might be tested and by the addition of water vapor, it was possible to test the same identical body of gas at a higher humidity.

To complete the description of the box, it is necessary to add that several small panes of glass were let into the sides, that the door opened the full area of one end, permitting a man to enter the box in a slightly stooping position, and that the box was set on legs with castors so that it could be moved about the laboratory readily. Every part of the box could be readily reached from the outside, thus permitting full control of leaks. Being exactly 100 cubic feet in contents, calculations proportionate to 1000 cubic feet of space were simplified. After use the gas was exhausted and driven out through one of the laboratory windows by an electric fan through a very large tin funnel.

Before giving the results of the tests made with this box, a study of the physical conditions under which formaldehyde is usually used may be briefly reviewed. This study related principally to formaldehyde released by heat from formalin at ordinary pressure and without the use of borax, glycerine, etc. A few experiments with paraformaldehyde were also made.

Formalin is the accepted name for a solution consisting of 40 grams of formaldehyde gas dissolved in water to make a total of 100 c. c. When standing in an ordinary narrow-mouthed bottle, with the stopper out, loss of percentage strength of formalin slowly occurs, without much loss of weight. In a wide-mouthed pan, formalin loses total weight by evaporation rapidly, but the percentage strength increases. After a time this concentration results in the formation of a solid cake of paraformaldehyde.

The specific gravity of formalin solution is greater than that of water, 1000 c. c. weighing approximately 1077 grams.

The boiling point of a 40 per cent. solution (vapor reading) is about 95° C. As evaporation and condensation goes on, the temperature rises, reaching 100° C. towards the end.

By attaching the outlet pipe of a flask or generator to a Liebig condenser, the vapor thrown off may be condensed. It becomes possible thus to measure accurately the rate of flow of various generators by collecting the distillate during ten minute periods throughout the runs. Analyses of these distillates by Romijn's method determines the amount of formaldehyde recoverable at various stages.

Using this method, it was found that the three generators tested each ran less than 15 c. c. per minute. In general, the rate was low at first, rising towards the middle of the run and lessening very much towards the end. The percentage of formaldehyde given off was low at first and increased steadily throughout the run. Conversely the proportion of water was greatest at first and diminished until at the end of the run, practically nothing but pure formaldehyde came over.

The solution itself, however clear at first, soon becomes cloudy from polymerization due to concentration, and towards the end, if evaporation be carried far enough, condensation and polymerization have gone so far that the still liquid residue will solidify on cooling, yielding a solid cake of paraformaldehyde. If this be heated further, it will be driven off as formaldehyde gas. The following selected experiments illustrate these points: (Since the Romijn process is not more accurate than will serve to distinguish solutions differing by not less than about 1 per cent., no attempt has been made to record results closer than this.)

First Experiment: Flask; 1 litre capacity; containing 900 c. c. formalin 40 per cent.; about 827 c. c. were distilled off; 10-minute samples were taken, percentage strength of which were as follows:

1st 10 min= 29%
2d 10 " = 30%
3d 10 " = 32%
4th 10 min= 34%
5th 10 " = 41%
6th 10 " = 50%

The distillation was then stopped. The residue in the flask was practically pure paraformaldehyde.

In evaporating a smaller quantity at about the same rate, the percentage of formaldehyde rises more quickly, thus:

Second Experiment: Flask; 1 litre capacity; containing 450 c. c. formalin; about 368 c. c. were distilled off.

1st 10 min= 27%
2d 10 " = 33%
3d (5 ")= 43%

The residue contained 77 per cent. formaldehyde.

It is quite evident, therefore, that a generator throwing out a given quantity of vapor in a given time does not throw out formaldehyde in the proportion in which the formaldehyde exists in the 40 per cent. solution. Thus, for the first experiment quoted, the amount of the distillate for each 10 minutes, and the number of grams of formaldehyde thrown out for each 10 minutes are given. For comparison are added figures showing the number of grams of formaldehyde in a quantity of formalin equal in each case to the distillate, and figures showing the relative strength of the distillate to the 40 per cent. solution:

Sample.	Amount distilled.	Grams formald. in distillate.	Grams formald. in same amount of formalin.	Ratio of strength of distillate to formalin.
1st 10 min.	126 cc	36 grams	50 grams	72%
2d 10 "	130	39	52	75
3d 10 "	168	53	67	80
4th 10 "	147	51	58	88
5th 10 "	136	56	54	104
6th 10 "	120	59	48	123

The total results achieved cumulatively at the end of each period follow:

10 min.	126 cc	36 grams	50 grams	72%
20 "	256	75	102	74
30 "	424	128	169	75
40 "	571	179	227	79
50 "	707	235	281	84
60 "	827	294	329	99

In the second experiment quoted above, the results were:

1st 10 min.	164 cc	44 grams	65 grams	70%
2d 10 "	139	45	55	80
3d (5 ")	65	28	26	108

The total results achieved cumulatively at the end of each period follow:

10 min.	164 cc	44 grams	65 grams	70%
20 "	303	89	120	74
25 "	368	117	146	80

So far the experiments quoted relate to formalin placed in an empty flask or generator.

It is sometimes attempted in practice to increase the percentage of formaldehyde thrown out in the early part of a run by leaving in the generator the residue from the previous run on the supposition that the paraform thus added to the total strength of the formalin evaporated would increase the amount of formalin thrown out. To test this, a generator was run five times, the residue from the first run being left for the second run, the residue from the second left for the third, etc. During the fifth run, ten minute samples were collected for analysis. The results were as follows:

Third Experiment: Generator; 4 litres capacity, containing 92 grams residue and 1000 c. c. 40 per cent. formalin solution; about 975 c. c. were distilled off; 10-minute samples were taken, the percentage strengths of which were as follows:

1st 10 min= 25%
2d 10 " = 27%
3d 10 " = 29%
4th 10 " = 31%
5th 10 " = 33%
6th 10 min= 39%
7th 10 " = 45%
8th 10 " = 48%
9th 10 " = 60%

The amounts distilled over, the grams of formaldehyde obtained and their relation to 40 per cent. formalin solution for 20-minute periods were as follows:

Sample.	Amount distilled.	Grams formald. in distillate.	Grams formald. in same amount of formalin.	Ratio of strength of distillate to formalin.
1st 20 min.	229 cc	59 grams	91 grams	65%
2d 20 "	223	69	93	74
3d 20 "	230	83	96	86
4th 20 "	216	96	86	111
5th (10 ")	67	40	26	154

The total results achieved cumulatively at the end of each period follow:

1st 20 min.	229 cc	59 grams	91 grams	65%
40 "	462	128	184	70
60 "	692	211	280	75
80 "	908	307	366	84
90 "	975	347	392	88

From these experiments, it is evident that to estimate the amount of formaldehyde thrown into a room from a generator by calculating as 40 per cent. formalin, the total difference between the original amount and the residue remaining is very likely to be wrong by about 20 to 25 per cent. unless the generator is run until the paraformaldehyde itself is evaporated.

Turning now to the results of the direct tests of the killing power of formaldehyde for the diphtheria bacillus under the conditions described in the first part of this paper, the experiments tabulated herewith are offered as evidence for the following general conclusions:

First—A large quantity of gas will kill very quickly even if the humidity is less than saturation. Small quantities will kill almost as quickly at the saturation point.

Second—The two main points in practical disinfection by formaldehyde are to get a sufficient body of gas into contact with the bacteria in a sufficiently humid atmosphere and to continue this contact until the bacteria are dead. In practice, these objects may be defeated (a) by using too little gas which may result from putting too little into the room or by losing too much of what goes in from leakage or wall absorption; (b) by having too low a humidity. This may likewise be due to putting in an insufficient quantity of water vapor or to excessively rapid loss of the same. (c) By uneven distribution of the gas, an excess accumulating at one point, leaving a deficiency at another. In practice these difficulties may be met more or less successfully (a) by stopping all leaks—a very difficult thing to do. Even if successful, absorption of the gas and vapor by the walls and by hygroscopic materials will not be prevented. (b) By putting into the room so great an excess of gas and water vapor as to make up for all losses and keeping the room closed long enough to insure equable diffusion of the gas and water vapor. Obviously a very large quantity of gas may be put into a room without effect, if it be put in so slowly that the inevitable loss prevents the total percentage at any one time from reaching the amount necessary for killing. Hence for the greatest economy of time and formaldehyde, the ideal conditions are (1st) to prevent loss by leakage, etc., as much as possible and (2d) to inject a percentage of gas accompanied by so much water vapor as shall ensure that at least the minimum killing quantity of the mixture shall reach every part of the room quickly and have a chance to act for the required time. Vaporization of paraform does not meet these requirements, for the gas is usually thrown off too slowly and the humidity is not increased to any important extent. Vaporization of formalin in the ordinary generator is too slow. It tends to raise the humidity, it is true, but not sufficiently for killing if the water vapor be derived only from the minimum necessary amount of formalin.

It is interesting to contrast the strength at which formaldehyde kills at the saturation point, with the strength at which it kills in solution. Thus 5 ozs. formalin per 1000 feet will kill at saturation point in 20 to 40 minutes. The gas is then present in the atmosphere so highly diluted with air that 100 c. c. of air contain only about 1-4500 grams of the gas. To kill diphtheria bacilli with formalin diluted with water in the same time it is necessary to have not less than ¼ gram formaldehyde gas to 100 c. c. of water, or 1000 times more gas in the same space. Thus it seems that the gas, accompanied by too little water, is relatively inefficient. If accompanied by a great excess of water it is also relatively inefficient. If accompanied by the proper intermediate amount of water its efficiency is greatest. There seems to be also a certain relation between the quantities of formaldehyde necessary to kill and the relative humidity. A great excess of gas will kill at comparatively low humidities, but to insure efficient action from small quantites, a high humidity is required.

One of the writers (Hill), therefore, proposes atomization of formalin either by a steam current, in which case the humidity required is furnished by the steam, or by an air current, in which case the humidity required may be furnished by diluting the formalin with water and atomizing the mixture. The steam or air current used, it is proposed, shall be strong enough to create decided currents in the air of the room, thus aiding diffusion and hastening the distribution of the gas.

In this laboratory, it has been found possible to run formaldehyde solution into a room by this method at a rate of 3 to 4 ounces (90 to 120 c. c.) per minute, using a steam current under about 25 pounds pressure, projecting the formaldehyde as a fine mist for 20 feet, and running 20 ounces and more at this rate without perceptible deposit of moisture on the floor. The humidity of the room is quickly raised to saturation. Since the formaldehyde is not heated, no polymerization occurs. For the same reason the formaldehyde thrown out can be accurately measured.

FORMALIN.

Number.	Grams of gas per 1000 feet.	Equiv. in oz. formalin per 1000 feet.	Fan running during exposure.	Time exposure, (minutes).	Greatest humid. during exposure	No. of rods exposed	Per cent. surviving.	Results in Detail.			Time to Growth.					More than 5 days.
								Positive	Negative	Contaminated	1 day.	2 days.	3 days.	4 days.	5 days.	
1	12	1	yes	20	95%	22	5%	1	9	12	—	—	—	—	—	1
2	12	1	yes	40	95 "	22	0	0	4	18						
3	12	1	yes	60	95 "	22	0	0	12	10						
4	12	1	no	60	90 "	44	18 "	8	6	30	—	1	5	1	—	1
5	12	1	no	20	100 "	10	30 "	3	5	2	—	—	2	—	1	—
6	12	1	no	40	100 "	10	0	0	8	2						
7	12	1	no	60	100 "	10	0	0	7	3						
8	36	3	no	20	100 "	10	0	0	10	0						
9	36	3	no	40	100 "	10	0	0	10	0						
10	36	3	no	60	100 "	10	0	0	8	2						
11	60	5		10	49 "	20	95 "	19	0	1	ng	19	—	—	—	—
12	60	5		20	49 "	20	95 "	19	0	1	ng	17	—	1	—	1
13	60	5		30	49 "	20	90 "	18	0	2	ng	17	—	1	—	—
14	60	5		10	95 "	20	60 "	12	4	4	—	3	2	2	—	5
15	60	5		15	95 "	20	30 "	6	6	8	—	—	1	1	—	4
16	60	5		20	95 "	20	15 "	3	13	4	—	—	—	3	—	—
17	60	5		20	100 "	10	10 "	1	9	0	—	1	—	—	—	—
18	60	5		40	100 "	10	0	0	9	1						
19	60	5		60	100 "	10	0	0	8	2						
19a	240	20	no	10	86 "	10	0	0	10	0						
						Paraformaldehyde.										
20	60	5	yes	20	42	16	100	16	0	0	10	6	—	—	—	—
21	60	5	yes	40	42	16	94	15	0	1	9	5	1	—	—	—
22	60	5	yes	60	42	16	100	16	0	0	7	6	3	—	—	—
23	60	5	yes	20	100	16	0	0	14	2	—	—	—	—	—	—
24	60	5	yes	40	100	16	?	?	13	2	—	—	—	—	—	1?
25	60	5	no	360	54	20	45	9	10	1	1	5	1	2	—	—
26	60	5	no	20	100	20	15	3	12	5	—	—	—	2	—	1
27	60	5	no	40	100	20	10	2	13	5	—	—	—	—	—	2?

Finally, results obtained in the formaldehyde box showed that in 30 to 40 minute exposure to formaldehyde thus atomized at saturation point the diphtheria bacilli are killed, when but one ounce of formalin per thousand feet was used. It is proposed to attempt the determination of the actual rate of leaking or absorption of formalin gas occurring in rooms, but for the present it seems safe to say that 5 ounces per 1000 feet used as recommended should hardly fail to make up for any loss occurring in a reasonably well prepared room.

EXPLANATION OF EXPERIMENTS GIVEN IN TABLES.

Experiments 1, 2, 3 and 4 were all made with the same body of gas, derived by boiling off formalin in the proportion of 30 c. c. per 1000 feet. The humidity was raised to 95 per cent. by boiling off water. Sixty-six rods, consisting of 3 sets of 22 rods each, each set made from 22 distinct cultures of *B. diphtheriae* on agar 24 hours old at 37° C., were placed in position, the formalin and water evaporated and the electric fan set going to secure a constant circulation of the gas throughout the experiment. One set of rods was removed at the end of 20 minutes. One rod only showed surviving bacilli and those were so far weakened that growth occurred only after five days. The rods exposed for 40 and 60 minutes yielded only dead bacilli.

After the removal of the third set of rods as above, the fan was stopped and 44 rods from the same 22 cultures (in duplicate) were inserted. The relative humidity had by this time fallen to 90 per cent. At the end of one hour, 8 rods showed bacilli still living, although somewhat weakened.

Experiments 5, 6 and 7, compared with experiments 1, 2 and 3, show the result of omitting the fan action while raising the humidity slightly. In experiment 5, 20 minutes' exposure gave a greater percentage of survival than in experiment 1, the growths occurring more quickly. In this experiment, the formalin was set free by atomization, as was also the water.

Experiments 8, 9 and 10 show the effect of a larger quantity of formalin at a high humidity. The fan was not used, yet all the bacilli were dead in twenty minutes.

Experiments 11, 12 and 13 show the effect of a still larger quantity of formalin at low humidity, 90 per cent. of the bacilli surviving 30 minutes' exposure and developing very promptly in striking contrast to the previous results. In experiment 25, an equivalent amount of gas derived from paraformaldehyde also at low humidity permitted the survival of 45 per cent. of the bacilli after 6 hours' exposure. It must be noted, however, that the results with paraformaldehyde, even at high humidity, were not quite so good as with equivalent amounts of formalin at similar humidity. (See experiments 23 and 24 and experiments 26 and 27.)

Experiments 14, 15 and 16 indicate the rate at which killing occurs during the first 20 minute exposure to 150 c. c. of formalin at high humidity. The time required for the survivors to develop lengthened as the time of exposure which they survived increased.

Experiments 17, 18 and 19 were confirmatory of previous experiments. Formalin and water in these experiments was atomized. Experiment 20 shows that an excess of gas will kill even at a relatively low humidity. It is worth noting that the results of experiments supposed to be conducted under the same conditions are not always identical. For instance, in experiment 17, one rod showed surviving bacilli after 20 minutes' exposure, although a similar experiment (No. 8) with a smaller quantity of gas showed no survival after the same length of exposure. Discrepancies of this character are probably due to the irregularity of the thickness of the films of bacilli on the rods exposed, a factor almost impossible to control.

These experiments do not include all those made, but only those necessary to elucidate the conclusions reached.

Note on humidity. The amount of water vapor suspended in a given amount of air may be termed absolute humidity. Such absolute humidity varies directly with the quantity of water vapor, the quantity of air remaining the same. For instance, of two bodies of air, each containing 100 cubic feet, that has the greatest absolute humidity which contains the greatest amount of water vapor without regard to any other factor.

Relative humidity is not wholly dependent on the absolute quantity of water vapor in the air. For each degree of temperature air will absorb only a certain maximum of water vapor, the amount increasing as the temperature rises. When any given body of air contains all the water vapor which it will absorb at that temperature, its humidity is said to be 100 per cent. If now the temperature rise, the amount of air and the amount of water vapor remaining the same, the amount of water vapor which the air can contain at the new temperature constitutes 100 per cent. humidity. So long as the amount of water vapor actually present remains what it was at first, the relative humidity at the new temperature must be below 100 per cent. Should the temperature fall to the old point, the relative humidity will rise to 100 per cent. without any other change. Barometric pressure affects humidity but slightly and may be neglected in practice.

It is this relative humidity and not the absolute humidity which affects the killing power of formaldehyde gas, according to Dudley and Marshall. The present writers have not yet repeated the work on this point at extreme temperatures.

If it be true, however, that on the relative and not on the absolute humidity depends the effectiveness of formalin, and that temperature has little if any modifying effect, then a rather curious result should be obtainable as follows, *i. e.*, an amount of gas which fails to kill in a given time at a high temperature because of low relative humidity might kill readily if only the temperature be chilled down far enough to ensure a rise in the relative humidity to saturation point, the actual amounts of gas, water and air remaining the same.

Elsewhere in this issue one of the writers (Hill) contributes work tending to show that the disinfection of rooms by formaldehyde after diphtheria is probably unnecessary. If, however, it be held that such disinfection is advisable, it seems best to disinfect in a manner which, while efficient, shall be as economical as possible in time and money.

AIR TESTING IN TUNNEL CONSTRUCTION.

By JOSEPH W. ELLMS, Cincinnati, Ohio.

The frequent resort to tunneling in modern engineering construction involves the necessity of furnishing by artificial means a pure, cool, and abundant supply of air for the men engaged in carrying on the work. The fumes arising from the use of explosives, the possible presence of inflammable gases, and the exhalations of the workmen, all tend to vitiate the atmostphere of the tunnel headings. These conditions require that effective methods of ventilation shall be employed to ensure the safety and health of the workmen.

In nearly all underground excavations carbon dioxide, marsh gas, and sometimes hydrogen may be present in greater or less amounts. Sulphuretted hydrogen occurs occasionally, and carbon monoxide may be present as the result of the combustion of inflammable gases and from the use of certain grades of explosives. Some of these gases, such as marsh gas and hydrogen, are dangerous because of their inflammable and explosive character when mixed with air and ignited by any means; some are poisonous to inhale, such as carbon monoxide and sulphuretted hydrogen; and others like carbon dioxide and nitrogen cause asphyxiation.

Excepting as the result of the combustion of inflammable gases, the exhalations of the workmen form the chief source of carbon dioxide, and ordinarily the ventilation of a tunnel has largely to do with the removal of the vitiated air thus produced. Ventilation is commonly effected by means of exhaust fans, blowers and air compressors; the latter also supplying air for operating drills at the face of the excavation and for compressed air when circumstances demand it.

A part of the new system of water works being built for the city of Cincinnati, Ohio, by the "Commissioners of Water Works," consists of a tunnel four and one-fifth miles in length, which is to convey the water from the reservoirs and filter plant to a distributing pumping station situated at the eastern end of the city. The tunnel lies about 120 feet below the surface of the ground, and has a diameter of seven feet in the finished brickwork. The diameter of the excavation averages between nine and one-half and ten feet. The portions of the tunnel thus far excavated pass through a limestone and shale formation.

From the first more or less gas of an inflammable and explosive character was encountered and several explosions occurred. It was, therefore, decided by Mr. G. Bouscaren, the chief engineer of the "Commissioners," to test the air of the various headings daily in order to detect inflammable and explosive gases, and also to determine how well ventilated the headings were being kept. The writer has had charge of this testing and the following description of the apparatus and methods employed may be of interest.

It was apparent from the first, that the ordinary methods of chemical gas analysis were not well suited to detect gases of an explosive character in the air of the tunnel with the rapidity that was necessary to make the tests of any immediate value. The use of miners' testing lamps of the Davy type by inspectors was considered unsafe and for certain reasons inadvisable. Our attention was called at this time to a mechanical device known as "Shaw's Gas Tester," which seemed to be able to give the necessary information, and which after some investigation was purchased.

The machine is capable of detecting quite small quantities of gases of an inflammable character, although it is not able to differentiate between them as a chemical analysis would do. It determines quantitatively the amount of explosive gas present and is sensitive to differences as small as 0.1 to 0.2 per cent. With this device samples of air can be tested very rapidly and it has proven to be well adapted to the case in hand.

The machine consists of two cylinders fitted with air-tight pistons. The pistons are attached by rods to one side of a beam which oscillates on a pivot. The larger of the two cylinders is situated at the extreme end of the beam; the other cylinder is movable and can be placed close to the larger cylinder or next to the pivot on which the beam turns. The beam is moved by means of a winch. The latter actuates gears which are connected by a rod to the end of the beam opposite from that to which the piston rod of the larger cylinder is attached. From the lower ends of the cylinders tubes lead to a two-way valve, which is turned automatically by the movement of the beam. From this valve tubes, so arranged as to mix the air and the inflammable gas pumped respectively through the large and small cylinders, lead to an explosion gun. This gun consists of a long, narrow cylinder, having at one end a movable head and at the other a cap provided with a small hole. A small tube projects from the side of this cylinder and connects with it through a small flap valve. The outer end of the projecting tube is immediately over the top of a Bunsen burner. Near the movable head of the gun is a gong, which is rung whenever an explosion in the gun is of sufficient force to drive out the movable head far enough to hit the gong.

A scale on the side of the beam, to which the piston rods of the cylinders are attached, is so graduated that when the piston rod of the movable cylinder is set to coincide with any division of the scale, the number corresponding represents the percentage by volume which the displacement produced by the piston of the small cylinder is of that produced by the piston of the large cylinder. As the position of the large cylinder is fixed and the movement of the piston always the same, the air which it pumps is a constant quantity. The piston of the movable cylinder, however, moves a longer or shorter distance, depending on its nearness to the large cylinder or to the pivot on which the beam is oscillating. It is evident, therefore, that by changing the position of the small cylinder varying proportions of any inflammable gas may be mixed with a constant volume of air, and may then be forced into the gun to determine whether the mixture is explosive or not.

The method of operating the machine is as follows:

A five gallon rubber bag is filled with illuminating gas and attached to the tube leading to the movable cylinder. The piston rod of the cylinder is set at about nine on the scale on the beam. The Bunsen burner is lighted in front of the ignition hole of the gun. By turning the winch the beam is made to oscillate. On the up stroke of the beam air from the room is drawn into the large cylinder, and illuminating gas into the small or movable cylinder. The two-way valve is automatically changed at the end of the up-stroke so that the ports open to the tubes leading to the explosion gun. On the down stroke the air from the large cylinder and the illuminating gas from the small cylinder pass into the gun well mixed, and the mixture is gently forced through the gun.

A portion of the mixture passes out the side tube of the gun over the top of the flame of the Bunsen burner and is ignited. At the end of the down stroke the ignited gas and the flame of the burner suck back into the main portion of the gun and ignite the mixture in the gun proper. If the mixture is of such proportions as to be explosive an explosion occurs, which drives out the movable head of the gun, causing it to strike the gong.

By gradually decreasing the percentage of illuminating gas added to the air, a mixture can be obtained of such proportions that the force of the explosion is not quite sufficient to cause the bell to be rung. Such a mixture is termed a "standard ringing mixture," and is determined for each set of tests made. It is obvious that, if now in place of the pure air of the room a sample of air containing gas of an inflammable character be pumped through the large cylinder, the gas contained in this air added to the known quantity of illuminating gas being used to form the "standard mixture," will cause a sufficiently strong explosion to ring the bell. By further decreasing the percentage of illuminating gas a point is again found where the force of the explosion is insufficient to cause the bell to be rung. If, for example, it was found that a standard mixture of air and illuminating gas, in which the latter formed eight per cent. of the whole, was barely sufficient to cause the bell to ring, and that when air containing inflammable gas was substituted for the pure air used in the "standard mixture," the force of the explosion was hardly great enough to cause the bell to ring with the addition of only six per cent. of illuminating gas, then it would be assumed that the difference between eight and six or two was the percentage of inflammable gas present in the sample of air being tested.

The samples of air from the various headings are collected in four gallon rubber bags. The bags, which are fitted with stopcocks, are pear-shaped and when empty their sides lie flat together. A small brass air pump is used to inflate the bags when taking samples of the air in the headings. The suction pipe of the pump can be placed at the roof, middle, or bottom of the excavation so as to obtain samples of the air from these different positions. The sample is usually taken near the top of the excavation so as to obtain the inflammable gases, which, because of their less specific gravity, lie near the roof of the tunnel. These samples are brought to the laboratory daily to be tested for explosive gases and for carbon dioxide.

The testing for carbon dioxide is carried out by the usual method of absorption of the gas in a barium hydrate solution. The air from the sample bags is transferred to calibrated bottles of about one liter capacity. Ten cubic centimeters of a standard solution of barium hydrate are run into the bottle, which is then closed with a rubber stopper. The bottle is then shaken several times to permit the carbon dioxide in the air to be absorbed by the barium hydrate. After standing from five to ten minutes the excess of barium hydrate is titrated with a standard oxalic acid solution. Corrections are applied for temperature and barometric pressure, and the results are stated in parts per hundred for standard conditions of temperature and pressure.

The present method of ventilating the various tunnel headings is by means of air compressors. They supply air under pressure for operating the drills working at the face of the excavation, and compressed air when needed in the course of excavating or lining. The air passes from the compressors to a receiver and is then conveyed by a pipe four inches in diameter to the bottom of the working shafts. The size of the pipe at this point is reduced to three inches. This three-inch line is laid along the bottom of the tunnel to the face of the excavation and supplies the air for the operation of the drills. The exhaust from the drills forms the source of the fresh air supply when the latter are in operation. When the drills are not in use the air is permitted to escape at intervals along the line as well as from the end of the pipe.

The explosive gases thus far encountered have generally entered the excavation at points where water was leaking into the heading in considerable quantity. It may occur in pockets and be liberated in the course of blasting; or it may permeate the rock in places and leak into the excavation because it is under a slight pressure.

An approximate analysis was made of the gas bubbling up through the water in one of the headings. It was found to contain 31.0 per cent. of hydrogen, 45.7 per cent. of marsh gas (methane), 2.2 per cent. of carbon dioxide, beside oxygen and nitrogen. The two latter gases were very likely of atmospheric origin, although the oxygen was in excess. There were also some indications of other hydrocarbons being present beside methane. In general the gas is similar in composition to the natural gas found in the Ohio and Indiana fields. It owes its explosive properties to the marsh gas and hydrogen which it contains.

The so-called "explosive limits" for mixtures of methane and air and hydrogen and air, as recently determined by Bunte and Eitner, are as follows:

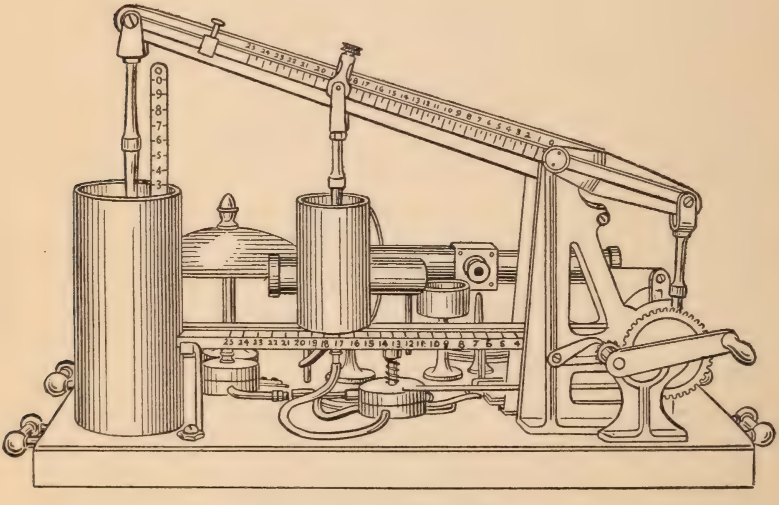
	Lower Limit. %	Upper Limit. %
Marsh gas (methane)	6.1	12.8
Hydrogen	9.4	66.4

It will thus be seen, that mixtures containing between 6.1 per cent. and 12.8 per cent. of marsh gas, and 9.4 per cent. and 66.4 per cent. of hydrogen, will form explosive mixtures with air. Other investigators vary these limits somewhat, and all state that the manner of igniting the mixtures affects the results obtained. Below the "lower limit" of explosion, mixtures will not explode, and above the "upper limit" no explosions occur, but the mixture burns freely where it lies in immediate contact with a layer of air which will furnish the necessary oxygen to support combustion. The chief danger from the presence of these gases in any quantity, lies in their rapid diffusion through the air with the consequent formation of mixtures of such proportions as will explode if accidentally ignited.

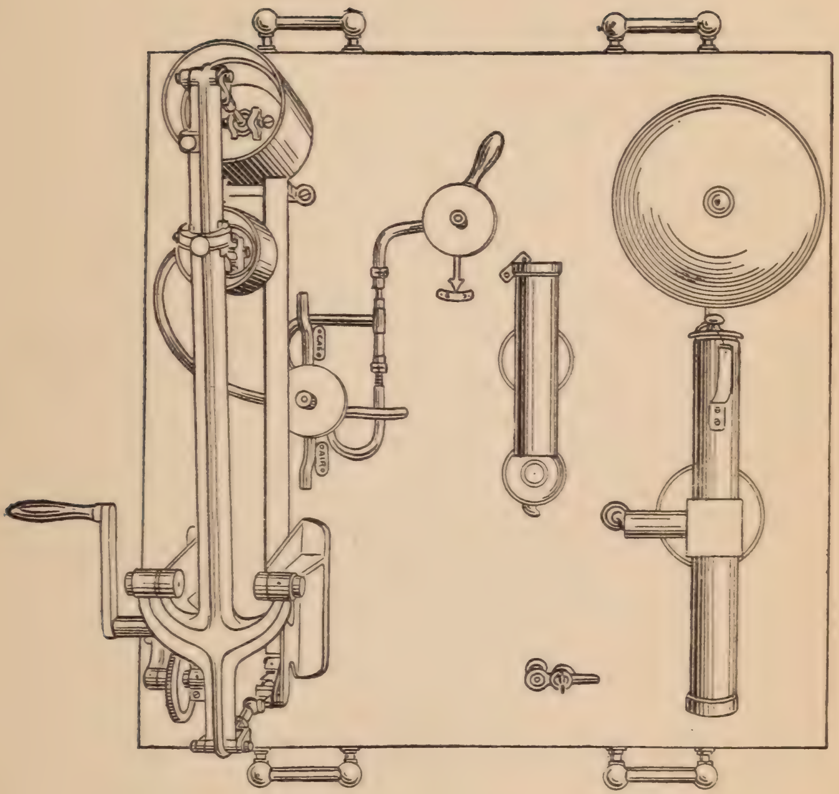
It was found that the "lower limit" of explosion of as pure a sample of the gas as could be obtained from one of the tunnel headings, was about 7.0 per cent. The "upper limit" was not definitely ascertained, but it appeared to be less than 20.0 per cent. These figures refer to tests made with the "Shaw Gas Tester" and to conditions such as exist in using this machine. This should be borne in mind if comparisons are made with figures obtained in more exact experiments. Moreover, it must be remembered that the explosive gas occurring in the headings is a mixture of gases and that its composition is probably variable.

In the course of the daily testing, samples of air containing as high as 67.0 per cent. of explosive gas have been found. Such samples could not be exploded directly, but burned freely when ignited. In order to obtain an explosive mixture in such cases dilution with air was necessary.

Traces of inflammable gas are not infrequently found. If present at all, however, it is usually in amounts of less than 1.0 per cent. The larger part of the time these gases are absent from the majority of the headings. But since it has been found that inflammable gases are liable to be encountered at any time, the only safety lies in a continuous testing of the air in order to immediately detect dangerous conditions. About 1700 tests for explosive gases and 1600 tests for carbon dioxide have been made during the past year.



Shaw's Indicator (Side View).



Shaw's Indicator (Top View).

A SIMPLE METHOD FOR RAPID AND ACCURATE WEIGHING.

By HOMER D. WILLIAMSON and ELMER G. HORTON,
Columbus, Ohio.

The chemical balance of today is a much used, and too, a much abused piece of apparatus, but withal an instrument of precision and absolute necessity.

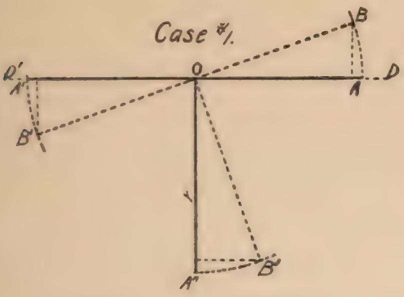
In most chemical laboratories, without doubt the balance is used more often, and during the course of a year a greater total number of hours, than almost any other piece of apparatus. So the time spent at the balance, especially if there is much "total solids," "suspended solids," and "sedimentation" work done, is very considerable. Hence, as great rapidity of manipulation as is consistent with accuracy is desirable on the score of time. In addition to the somewhat mercenary item of time, it is evident to every practical chemist that for the accurate weighing of many substances, rapidity of manipulation is also essential.

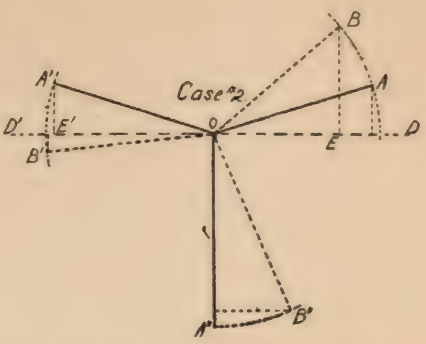
For instance, the total mineral content of Ohio ground waters is rather high, often a "total solids" of as much as 800 to 1200 parts per million being found. Often the "total solids residue" from such waters is very readily deliquescent; and in weighing such a residue by the "cut and try" method considerable error is sometimes introduced. In a laboratory provided with an appropriate balance room, this difficulty would probably not have occurred.

To obviate such errors in weighing deliquescent substances, the following method was devised. As its use saves approximately 50 per cent. of the time formerly required at the balance, in our laboratory we would not think of returning to our former method.

First, we will take up the elementary theoretical considerations upon which the method is based, and then, the practical considerations:

Case No. 1. A balance in which the three knife-edges are in a horizontal plane.





The angles AOB, A′OB′, and A″OB″ are equal and designated as the angle "h."

The length of each arm is designated as "l."

From the central knife-edge to the center of gravity as "r."

Load as "P." Excess weight on OA′ as "p."

Weight of the balance system as "w."

Then, pl COS h equals wr SIN h

$$\frac{pl}{wr} = \frac{\text{SIN } h}{\text{COS } h} = \text{TAN } h.$$

The only variable is "p"; hence TAN h varies directly as "p".

Case No 2. A balance in which the two end knife-edges are in a horizontal plane, the central knife-edge being in a lower plane.

The angles AOD and A′OD′ are equal and designated as "k."

Other specifications the same as in Case No. 1.

Evidently with an excess "p" on the left arm, the moment arm of A′O is increased to E′O, while the moment arm of OA is decreased to EO. Consequently for a given excess "p" on the arm OA′, the angle of deflection or "sensibility" is greater than when AOA′ are in a straight line. Evidently too, the arm of OB′ will decrease after the angle of swing, "h," becomes equal to "k"; but the moment arm of B′O will be greater than of OB, until OA would swing through an angle of 90°.

Or proven mathematically:

$$wr\,\text{SIN } h + Pl\,\text{COS }(h+k) = \pm (P+p)l\,\text{COS }(h-k)$$

$$wr\,\text{SIN } h + Pl\,(\text{COS } h\,\text{COS } k - \text{SIN } h\,\text{Sin } k)$$

$$= Pl\,(\text{COS } h\,\text{COS } k + \text{SIN } h\,\text{SIN } k) + pl\,\text{COS }(h-k).$$

$$\text{Hence, } wr\,\text{SIN } h = pl\,\text{COS }(h-k) + 2Pl\,\text{SIN } h\,\text{SIN } k$$

For all values of "h" and "k," $2Pl \sin h \sin k$ will be +

Also, $\cos(h-k)$, since both "h" and "k" will necessarily be less than 90°, is > either $\cos h$ or $\cos k$.

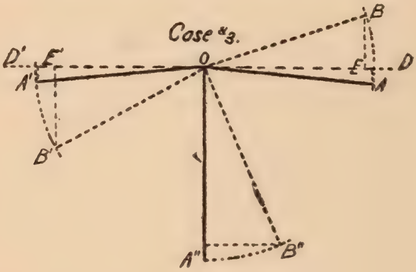
Hence, $pl \cos(h-k) + 2Pl \sin h \sin k > pl \cos h$.

Therefore, $wr \sin h = pl \cos h + x$

$$\text{and, } \tan h = \frac{pl}{wr} + X'.$$

Interpretation shows that with the same excess weight "p," the angle of deflection will be greater in Case No. 2 than in Case No. 1. However, in both cases TAN h varies directly as the variable "p."

CASE NO. 3. A balance in which the two end knife-edges are in a horizontal plane, the central knife-edge being above this plane.



Specifications the same as in Case No. 2.

$wr \sin h \pm Pl \cos(h-k) = (P+p) l \cos(h+k)$.

Therefore, $wr \sin h = pl \cos(h+k) - 2Pl \sin h \sin k$.

Now, $\sin h \sin k$ will always be + ; also $\cos(h+k) < \cos h$.

$\therefore wr \sin h = pl \cos h - x$

$$\text{Hence, } \tan h = \frac{pl}{wr} - X'.$$

Interpretation shows that in this case the angle of deflection (h), due to excess weight "p," is less than when the three edges are in the same horizontal plane; but as in both preceding cases, TAN h varies directly as "p."

Or by inspection, when the balance arms are rotated through an angle "h," the moment arm of OA will (with "h" having any value < 90°), be greater than the moment arm of OA′. Hence, for a given excess weight on the left pan, the angle of deflection will be less in this case than in the case of a balance with the three edges in a horizontal plane.

Summarizing: For a given excess load on one arm, the angular displacement of the indicator will be greatest in Case No. 2, least in

Case No. 3, and intermediate in Case No. 1. Moreover, if the excess weight "p" be doubled, trebled, or increased by any amount, TAN h will be increased proportionately. This means that by finding the angle of deflection with a certain load "P" on the pans, for excess weights of say 1 mg. and 3 mg. respectively, a curve can be plotted from which the excess weight represented by *any* angle of deflection can be read at a glance. (The angle of deflection is limited only by half the angle subtended at the central knife-edge by the indicator scale.)

This discussion makes clear at least one cause for the change of sensibility with change of load seen in some balances. If the beam be not sufficiently rigid, a change in the sensibility may result from a bending of the knife-edges out of their proper relative positions when the load is sufficiently increased.

No discussion need be made of the correction for unequal balance arms, as, for any but exceptional work, a good chemical balance will possess sufficient accuracy of adjustment. A few minutes will suffice to determine the relative lengths of the arms by placing a small weight, say 2 mg., on the two pans successively, and noting the deflection in each case by the method to be described.

This method is based upon the physical fact before discussed, viz., that for a given balance, the amount of deflection is directly dependent upon the excess of weight upon one arm.

As found from theoretical considerations we would need to work with the TAN of the angle deflection. Not so in practice. In case of the balance used here in the laboratory, the indicator arm is about 9½ inches long, while the whole scale length is about .9 of an inch. But take the more unfavorable dimensions of 7 and 2 inches respectively. The maximum deflection possible is then over a scale length of 1 inch, corresponding to an angle of approx. 8° 15′. On our balance the angle of deflection due to .5 mg. is very nearly 30′.

.008727 is the natural TAN of 30′. Then, for 1.0 mg. the TAN h would be .017454; for 2.0 mg. or four times the original excess weight it would be .034908, etc. Column No. 1 of the following table represents these values. Column No. 2 represents the angle in minutes corresponding to these tangents. Column No. 3 represents once, twice, four times, etc., 30′. Column No. 4 represents the values obtained by dividing column No. 2 by column No. 3. Column

No. 5 represents the per centage error if "h" is used instead of TAN h of theory.

Column No. 1.	Column No. 2.	Column No. 3.	Column No. 4.	Column No. 5.
.008727	30.000	30	1.00000	0.0000
.017454	59.996	60	.99993	0.01
.034908	119.955	120	.99962	0.04
.052362	179.842	180	.99912	0.09
.069816	239.620	240	.99842	0.16
.087270	299.253	300	.99751	0.25
.104724	358.747	360	.99652	0.35

It is seen that even for excess weights of as much as 6.0 mg. the error is well within the error of manipulation. So, henceforth, we will use "h" entirely instead of TAN h, of theory; this will simplify matters very much.

In practice it is seldom desirable to use the vibration method for excess weights of more than 2.5 mg., it being very easy to use the 5.0 mg. weight or the rider within 2.5 mg. of the proper weight. So this makes the maximum relative error about .15 per cent. This means .15 per cent. of the deflection which in most cases will not be more than 30 or 40 interpolated divisions, dividing each marked division of the scale into 10 interpolation divisions; .15 per cent. of 40 is .6 of an interpolated division; this, too, is well within the error of reading. So there is no accuracy sacrificed by reckoning that the deflection on the scale is directly proportional to the excess weight, and vice versa.

Plotting the Curve. First, determine the "zero point," *i. e.*, the middle of the swing with nothing on either arm. Then move the rider to the point corresponding to a weight of 1.0 mg. and observe the number of interpolated divisions that the center of the swing is deflected from the "zero point." Make at least three sets of readings, thus:

"Zero point," no load.

82	118	84	116	81	119
83	117	84	115	82	118
84	116	85	115	83	117
84	116	85	114	84	116
85	115	86	114	84	116
——	115	——	113	——	115
Av. 83.6	——	Av. 84.8	——	Av. 82.8	——
Av. 99.90	116.2	Av. 99.65	114.5	Av. 99.80	116.8

"Zero Point" 99.78

No load on pans; rider indicating 1.0 mg. on the left arm.

79	148	82	145	82	144
80	146	83	144	83	143
82	145	84	143	84	142
83	144	85	142	85	141
84	143	86	141	86	140
——	142	——	140	——	138
Av. 81.6	——	Av. 84.0	——	Av. 84.0	——
Av.113.10	144.6	Av.113.25	142.5	Av.112.65	141.3
					Total av. 113.00

Therefore, with no load, the deflection due to an excess weight of 1.0 mg. is 13.22 interpolated divisions. (These readings were made on a scale of 200 interpolated divisions; the left end is termed "0," the center "100," and the right end "200"; hence the above readings.)

Determine successively the deflection due to an excess of 1.0 mg. for loads of 5, 10, 15, 20, 25, etc., grams. The "zero point" will have to be determined for each load as it is likely that the weights used will not exactly counter-balance. If the balance is inaccurate, it will be well to determine also the deflections due to excess weights of 2.0 and 3.0 mg. respectively. (This preliminary work is rather tedious, and will probably require two or three hours' time.)

Suppose the following results are obtained for an excess weight of 2.0 mg. Column No. 1 denotes the load on each pan, in grams. Column No. 2 denotes the deflections found, in interpolated divisions. Column No. 3 denotes the deflections due to 1.0 mg. excess weight, obtained by dividing the determined results by 2. Column No. 4 denotes the reciprocals of the figures of Column No. 3, *i. e.*, the excess weight represented by a deflection of one interpolated division.

Column No. 1.	Column No. 2.	Column No. 3.	Column No. 4.
0	24.90	12.45	.0803
5	25.00	12.50	.0800
10	24.84	12.42	.0805
15	24.76	12.38	.0810
20	24.96	12.48	.0800
25	25.04	12.52	.0800
30	25.12	12.56	.0796
35	25.20	12.60	.0794
40	25.04	12.52	.0800
45	24.98	12.49	.0800
50	25.08	12.54	.0797
55	24.98	12.49	.0800
60	24.92	12.46	.0803
		Average	.08006

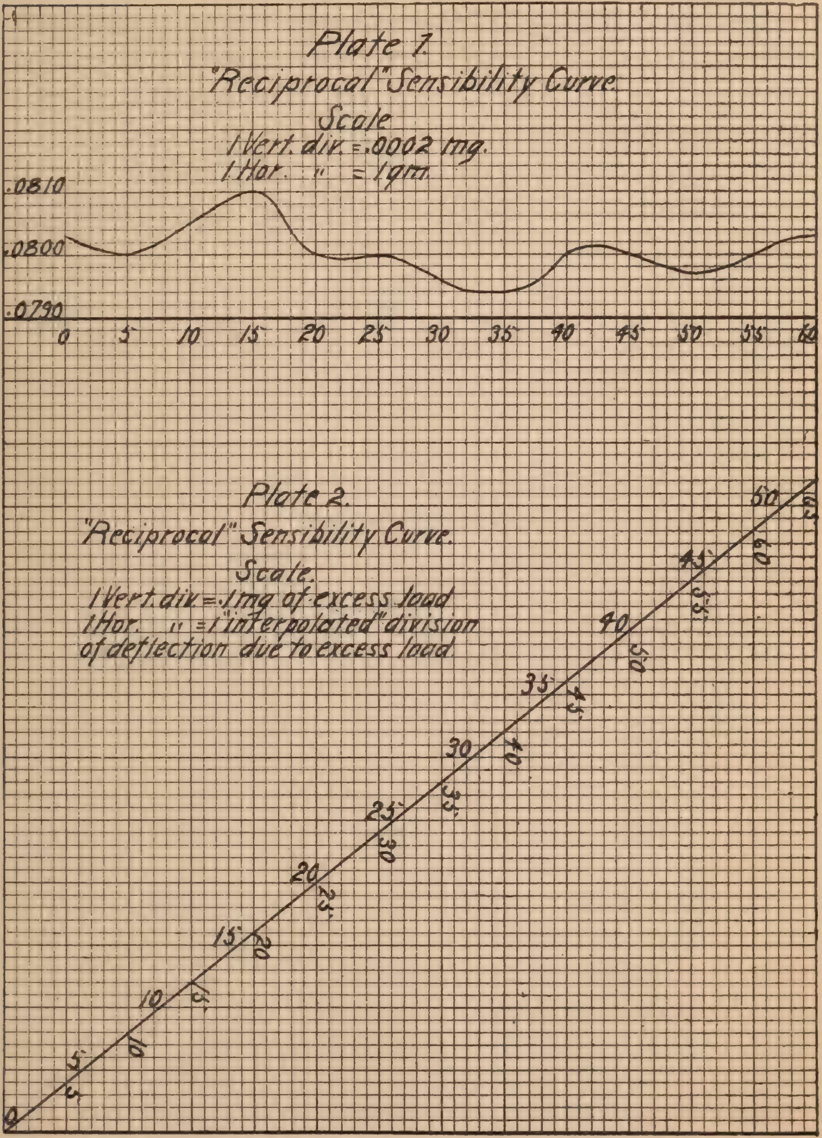
In Plate No. 1 the results obtained in Column No. 4 are plotted. The ordinates indicate the excess weight corresponding to a deflection of one interpolated division with various loads on the pans; which loads are represented by the abscissae. Theoretically this curve should be used, but practically, as a deflection of more than forty divisions will seldom be observed, the error resulting from using a straight line curve representing the average of the results in Column No. 4 is very small.

Hence, for practical purposes, the curve on Plate No. 2 can be used. In this curve the ordinate indicates the excess load represented by the deflection plotted as an abscissæ. For instance, suppose an evaporating dish is being weighed. The weight on the right pan is 36.1950 gm., and the deflection is 29 interpolated divisions to the right. By examining Plate No. 2 we see that 29 divisions correspond to an excess load of .0023 gm. Hence the weight of the dish is 36.1973 gm.

In using this method the "zero point" with no load should be determined each day the balance is used. Unless the substance to be weighed is very deliquescent it is well to let the indicator make one complete cycle before taking readings. But if extreme rapidity is desired the first two readings may be taken; the error will be almost within the error of reading. Theoretically, at least five readings at one end of the swing, and four readings at the other end should be taken, as was done in obtaining data for the curve. Practice shows, however, that it is entirely satisfactory and accurate to proceed as above described, making but two readings.

If the balance is too inaccurate to make a straight line curve practicable for all loads, it will very seldom be the case that a straight curve cannot be drawn at least for any load of 30 to 40 gm., which limits should include the weights of all evaporating dishes.

Another practical point in rapid weighing, applicable to one phase of water analysis, is to have at least 36 evaporating dishes in commission; for the 36 dishes say 6 dessicators, numbered from 1 to 6 inclusive. Before numbering the dishes, determine the approximate weight of each dish. Arrange these weights in the most economical order, *i. e.*, so that in weighing the whole set the number of weights to be changed will be a minimum. Then number the dishes from 1 to 36 inclusive so as to preserve this most economical order. Before removing a dish from the dessicator, place on the balance the approximate weight of the dish; then remove the dish from the dessicator to the balance, close the balance case, drop the levers from under the pans, carefully lower the load upon the knife-edges, take the two readings of the second cycle; mentally determine the deflection, from the curve read off the corresponding excess weight which must be added to or subtracted from the weight on the pan.



COLLODION SACS.

By W. D. FROST, Madison, Wis.

The collodion sac was first used in work on the toxins and antitoxins of cholera by Metschnikoff, Roux and Salimbani in 1896, and later by Nocard and Roux in their work on pleuro-pneumonia in cattle. The exact method of its manufacture, however, was not published until Novy described it in his "Laboratory Work in Bacteriology" in 1899. This was an exceedingly difficult technique. Within the past few years, however, a number of procedures have been published which very much simplify the method of making it and will do much to secure its more general use.

In all of the methods thus far proposed for making a sac, for bacteriological use, the sac has been molded on the outer surface of a pattern made either of glass or some soluble material as gelatin. Of those molded on the outer surface of a plain glass tube or rod the Paris system (1, 2, 3 and 4) should be mentioned first. This is used by the French school and is described by Novy. The sac is formed by rolling the tube in collodion, hardening it in water, peeling it off and then attaching it to the end of a constricted glass tube which is sealed off just before the sac is placed in the animal.

Gorsline (5) in 1902 suggested the use of a tube having a small opening at the end which facilitates the removal of the sac.

Ruffer and Crendiropoulo (6), 1900, recommended that the sac be loosened from the tube by alternately dipping it into alcohol and into water. They also state that the sac may be closed by ligating it with a sterile thread just before it is inserted into the animal, thus obviating the necessity of sealing on and fusing a glass end.

Another system is characterized by the fact that the sac is molded over a perforated glass tube from which the sac is not to be removed. This method was described by Grubbs and Francis (7)) in 1902. It obviates the necessity of removing the sac from one tube and sealing it on another and gives the sac strength and rigidity.

In a third system the sac is molded over gelatin capsules. The first method was described by McCrae (8) in 1901. The gelatin capsule is sealed to a glass tube before it is dipped in the collodion. After the sac is hard the gelatin is dissolved out. This leaves the sac attached to the tube ready for use.

Harris (9), 1902, modifies McCrae's method so as to increase the dialysing surface, but what is more important, he points out the necessity for air drying the film, which he says produces "a thinner, tougher, less shrunken and equally good dialysing membrane."

The method about to be described was devised in the course of some work requiring a large number of sacs free from gelatin or other material which would serve as a culture medium. The experience of several months with these sacs which were made for a special purpose indicates that they are suited for general use. The technique is very simple and seems to combine many of the good points of the other systems.

In this system the sac is formed on the inside of a tube and allowed to air-dry. The shrinking of the sac as it dries tends to loosen it from its mold rather than tighten it as in other systems. This method of molding the sac has recently been used by Karl Kellerman for making tubes for dialysing and was recently described by him (10). When the sac has been sterilized, inoculated and tested it is ligated and is then ready to be inoculated into the animal.

The detailed method of procedure is as follows:

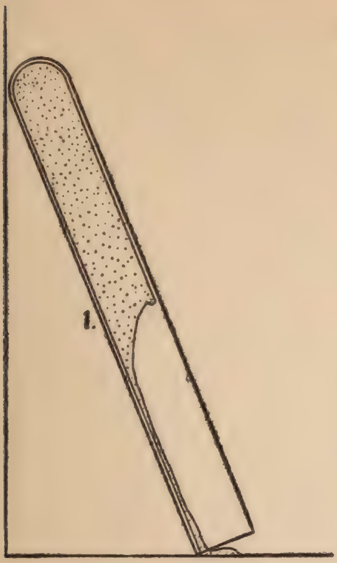
Forming the Sac. Glass tubes are selected, of any desired size, with evenly rounded bottoms. Usually a small-sized test-tube will be found to be of the right size. Thick collodion is then poured into the tube to a depth equal to the desired length of the sac. The collodion is then poured out along one side of the tube into another tube and from this one to another and so on until it is all used up or becomes filled with bubbles. The desired length of the tube can be secured in all of the tubes by tipping and rolling them, thus bringing the collodion into contact with the glass to the desired height. As the tubes are coated they are placed, mouth down, in a wire basket or test-tube rack as indicated in Fig. 1. In this way the extra collodion drains off and free access of air dries and hardens the collodion, leaving a thin coat covering the inner surface of the tube. The thickness of the coat depends on the consistancy of the collodion. A ten per cent. collodion, in equal parts of alcohol and ether, makes a sufficiently thick coat for ordinary purposes. Kellerman uses only a three per cent. solution to make tubes for chemical purposes. He points out also the facts that a thicker solution makes a stronger but slower dialysing sac and longer air drying makes a tougher but slower one. It seems necessary to have a tough sac since it has been found that bacteria will sometimes pass through a sac which will hold water. The collodion is allowed to air-dry from a few minutes to several hours. When thoroughly air-dry the sac usually shrinks from the tube and may be easily pulled out. The drying may be stopped at any point by filling the tube with water and after standing a few minutes the collodion shrinks and the sac may be easily removed. Should cold water fail to loosen the sacs warm water will. There is very little danger from bubbles in this method as in the older ones, since any which form either spontaneously rupture or settle towards the mouth of the sac and are later cut off. According to this method a large number of sacs may be made in a short time. They may be kept for a long time in water, but become brittle if allowed to dry after they have once been hardened in water.

Sterilization of Sac. The sacs are filled from one-fourth to three-fourths full with bouillon or other culture medium if desired. They are then immersed in a test-tube of the medium. The sacs are held in position in the test-tube by means of the tongue formed by the collodion flowing out of the tube. This tongue is folded over the lip of the tube. (Fig. 2, C.) Before, however, the sac is put in the test-tube a piece of cotton or silk cord is placed around the sac near the top and held in position by means of a surgeon's knot, loosely drawn. The cord should be quite stout so that the sac can later be tightly closed. The ends of the cord are brought outside of the tube as shown at B, Fig. 2. Sterilization may be accomplished either in the autoclav or by means of the intermittent method of sterilization.

Inoculation and Testing of Sac. The medium is inoculated by means of the platinum needles in exactly the same way in which tube cultures are ordinarily inoculated. The tube thus inoculated should be incubated for twenty-four hours and if the medium outside of the sac remains clear the sac may be used. Otherwise it would be discarded. This testing of the integrity of the sac is necessary whatever method of making it is employed.

Sealing the Sac. The tube is placed in a tumbler or test-tube rack. The sac is then pulled out of the tube until the cords can be drawn tight so as to close the sac and securely tied. With sterile scissors the end of the sac is cut off a few millimeters above the constriction. If there is any moisture on the inside of the sac above the neck this must be removed with sterile filter paper and then a few drops of a thin solution of collodion is placed in the neck so as to hermetically seal the sac. The long and contaminated ends of the cord are now cut off, the sac dropped back into the test-tube, and the cotton stopper replaced. (Fig. 3.) The sac is now ready to be placed in the body cavity of an animal. The method of procedure here is of course the same as in any other method.

Advantages of this method:
First—Simplicity.
Second—No danger from air bubbles.
Third—May be made of any size or shape.
Fourth—No glass to break or irritate the animal.
Fifth—Maximum amount of dialysing surface.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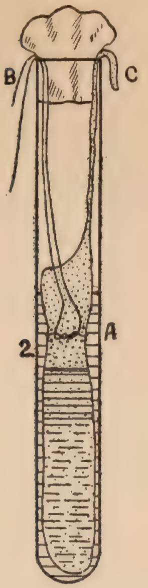
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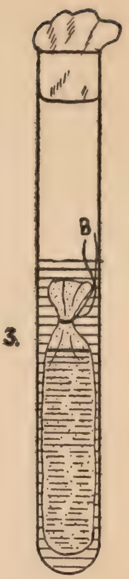
3.

LEGENDS.

Fig. 1. Tube inverted to allow the extra collodion to drain off and the film to air-dry.

Fig. 2. Sac ready for sterilization. (A) Surgeon's knot. (B) Ends of cord. (C) Tongue of collodion.

Fig. 3. Sac ready to be inoculated into animal.

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ON THE GERMINATION OF THE SPORES OF B. SUBTILIS AND B. MEGATHERIUM.

By LEO F. RETTGER, New Haven, Conn.

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This paper is of the nature of a preliminary communication, as the experimental work upon which it is based is as yet incomplete.

Bacteriological text-books agree in saying that the most common process of spore germination is that which is illustrated in the sprouting of the spores of *B. ramosus*, *B. amylobacter*, etc. The spores swell, lose their strong refractive power and germinate by bursting through the membrane, at or very near the polar extremity of the spore. As the new bacillus grows its length rapidly increases, and the spore membrane is seen to remain for a while attached to one end of the bacillus, as a capsule. Gradually this capsule backs away from the bacillus, finally slipping off and floating about freely in the medium.

B. subtilis and *B. megatherium* are usually cited as peculiar exceptions of the above process. Brefeld,[1] Prasmowsky,[2] De Bary[3] and others believed that the sprouting of the spore of these species did not take place through a polar, but through an equatorial rent in the spore membrane; thus presenting a very different appearance from the germinating spore of *B. ramosus*. In the latter species the main axis of the new bacillus is parallel with that of the original spore; while in the case of *B. subtilis* it is secured as perpendicular, or nearly so, to it.

Whether the process involved in the germination of *B. subtilis* actually is as described by Brefeld and others, is a matter of considerable scientific interest, and forms a part of the work now in progress, of which this is a preliminary report.

A second question presents itself, namely: Does *B. megatherium* follow the equatorial mode of spore germination that has been ascribed to *B. subtilis*, or does it conform to the more common polar type? Observations on *B. subtilis* have been rather numerous; but concerning *B. megatherium* the work has been more fragmentary, and perhaps largely inferential, owing to the similarity in general between the two organisms.

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[1] Brefeld. Botanische Untersuchungen, 1881-4, p. 43.

[2] Prasmowsky. Entwickelungsgeschichte und Ferment—wirkung einiger Bacterien-Arten, Leipsic, 1880.

[3] De Bary. Vergl. Morphologie und Biologie der Pilze, 84.

The spores of megatherium bacilli from five different sources were examined by me. In their common diagnostic characteristics the bacilli in each case answered the description of true megatherium organisms. In every instance, however, the germination of the spores was observed to proceed in the common way, as exemplified by *B. ramosus*, etc. There was no indication of equatorial splitting of the membrane or of sprouting from the side of the spore. Further, spores from three out of five different cultures of supposed *B. subtilis* showed the same method of germination. After careful diagnosis, however, the bacilli from these three cultures were found to correspond to the description of true megatherium bacilli, and had evidently been mistaken for *B. subtilis*.

If, on the other hand, these organisms were not *B. megatherium*, but merely varieties of *B. subtilis*, then it is evident that the process of spore germination of this species of bacteria is of two kinds, according to the variety. It is more probable, however, that the cultures of supposed *B. subtilis* were in reality cultures of *B. megatherium*. If this is true, then, in the light of the present observations, *B. subtilis* and *B. megatherium* represent distinct and definite types of spore germination, and can readily be distinguished, the one from the other, on this basis.

The "hanging block" method of Hill[1] is an excellent device for the study of spore germination, and it is due to its efficiency that I have been able to observe the above processes with the degree of clearness and satisfaction that was attained. The older hanging drop (bouillon, gelatin and agar) methods offer many obstacles in this kind of work; obstacles which are easily, and almost completely, overcome by the new method of Hill.

NOTE.—The observations reported in this paper were begun in the bacteriological laboratory of the Boston Board of Health.

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[1] Hill. Journal of Medical Research, 7, 1902, p. 202.

THE SUPPOSED IDENTITY OF HUMAN AND AVIAN DIPHTHERIA.

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Diphtheria of the lower animals, especially of fowls and pigeons, has been made the subject of numerous investigations; and when we examine the literature, we are immediately struck with the differences of opinion regarding the disease. On one side, we have those who believe that the one disease in man and in birds is identical; and, on the other side, those who believe that the one disease has no relation to the other. If the two diseases are identical, that is, if both are caused by the same organism (the Klebs-Löffler bacillus), we should have to demand the most rigorous treatment of diseased birds, and the disease would hold a place of importance as high as that now accorded to bovine tuberculosis. But does our experience of the occurrence of fowl diphtheria and diphtheria in the human being lead us to the conclusion that there is a probability of there being any connection between the two diseases? Is not diphtheria a disease which is more prevalent in towns and closely populated districts, where very few fowls are kept? Is not roup at certain seasons almost universally prevalent in some poultry yards in the country, where there may be hundreds of cases of the disease without a single case of diphtheria among those who look after the birds and treat them from day to day? Has it ever been observed that poultrymen are more frequently attacked with diphtheria than those who keep aloof from poultry yards? These, and other questions, suggest themselves to any one who critically examines the problem.

Let us, however, examine the work of various writers and investigators on this subject, which we may conveniently group under two heads:

1. Those who have investigated the disease as it occurs in fowls and pigeons, by the usual methods employed in working out infectious diseases.

2. Those who have made observations without experimental research, and who did not employ bacteriological methods to support or controvert their views, either for or against the identity of the disease as it appears in birds and man.

1. *Experimental Research.* Rivolta (1), Friedberger (2), Silvestrini (3), and Davaine (4) made a number of microscopical observations on the disease prior to the introduction of the bacteriological methods of Koch and Pasteur, and hence I shall not dwell upon their results, but pass on to the work of Löffler (5) in 1884, on the diphtheria of pigeons and chickens.

From diphtheritic pseudo-membranes in the mouths of pigeons which died from an infectious form of diphtheria, which prevails in Germany, Löffler isolated a bacillus which, when inoculated in pure culture into pigeons produced exactly the same symptoms as those which are met with in birds that acquire the disease naturally. This organism was quite different from the true diphtheria germ, and was not pathogenic for guinea pigs, rats or dogs. Loeffler's discovery was subsequently confirmed by Cornil and Megnin (6).

Chicoli (7) and subsequently Krajeweski (8) found cocci in the false membranes of fowls; and Perroncito (9) saw coccidia in those of pigeons, chickens and turkeys.

In 1889, Pfeiffer (10) from fowls and pigeons attacked by diphtheria and contagious epithelioma observed corpuscles having ameboid movements, which he considered were gregarines, and in other cases discovered flagellata which were difficult to distinguish from leucocytes.

The following year, Babes and Puscariu (11) found *Trichomonas* showing the same characters as those described by Pfeiffer, but in diseased pigeons, these protozoa were associated with the bacillus discovered by Löffler, and which alone was able to produce the disease. Thus, the *Trichomonas* must be looked on as being normally present in the pharynx of pigeons.

In 1894, Loir and Ducloux (12) studied in Tunis, an epizootic of diphtheria which affected fowls, ducks, sparrows, pigeons and turkeys. From all these cases they isolated a motile bacillus with rounded ends, which gave yellow colonies on gelatin, and did not stain by Gram's method. As cases of diphtheritic angina were frequent among those who cared for the fowls, Loir and Ducloux made bacteriological examinations of such persons, and isolated in one case the same bacillus as was found in the diseased birds. The inoculation of this bacillus into healthy fowls gave them the disease.

The same year, Piana and Galli-Valerio (13), whilst studying an epidemic of pigeon diphtheria, found large motile corpuscles, 4 micron in diameter, which these authors considered as protozoa.

In America, Veranus Moore (14) has isolated a bacillus from the false membranes of fowls, which resembled the organism causing swine plague. Moore inoculated a culture of this germ into a young hen, which died, but without showing false membranes. Aged fowls were quite refractory to inoculation.

Mazzanti (15) and Piana (16), and others, have confirmed the observations of Pfeiffer, with regard to the presence of flagellata, either alone, or accompanied with micro-organisms.

In 1898, as the result of experiments on diseased fowls, with diphtheritic antitoxin, Stevenson (17), of London, Ont., recommended the use of this serum for the treatment of diseased birds, and stated that "roup," the popular name for fowl diphtheria, was "caused by a specific germ, which appears to me to be identical with the Klebs-Löffler bacillus, and that roup and canker were the same disease, a disease identical with diphtheria in man."

In 1899, McFadyean and Hewlett (18) found bacteria resembling the Klebs-Löffler bacillus in the throats of pigeons suffering from canker, but they demonstrated that the disease could not be produced in healthy birds by the inoculation of pure cultures of this germ, and they showed that it had no toxic effect upon mice and guinea pigs, such as that produced with *Bacillus diphtheriae*. Therefore, they concluded that it was not the same organism.

I (19) have also obtained a germ resembling the Klebs-Löffler bacillus from the throats of pigeons which were perfectly healthy before examination, and remained so afterwards.

The above are a few of the numerous works relating to the etiology of fowl diphtheria. Let us now examine the opinions of some of those writers who favor the identity of human and avian diphtheria.

Buniva supported the first hypothesis of the identity of the two diseases. In 1879, Nicati (20) reported that cases of human diphtheria, especially of the conjunctiva, increased after epizootics of avian diphtheria. Menzies (21) announced that at Posillipo, cases of diphtheria were noticed among children who drank water which flowed over a terrace occupied by pigeons and fowls. Gerhardt (22) published the report of an outbreak of diphtheria among two-thirds of the employes of a poultry establishment at Nesselhausen, where thousands of fowls had succumbed to diphtheria. One of the employes, pecked by a diphtheritic rooster in the hand and foot, subsequently showed false membranes in these locations. No cases of diphtheria were present in the environs of Nesselhausen at the time. Roth (23) reported that fowls which had swallowed the false membranes of diphtheritic children were affected by this disease.

Wheler (24) observed diphtheria in a family which had eaten doves affected with diphtheria. Turner (25) has stated that infant croup has been preceded by fecal diphtheria.

According to Paulinis (26), the Greek isle of Skiatos, in which diphtheria had never previously been known, was infected after the introduction of diphtheritic turkeys. Boing (27), Hingworth (28), and Bilhaut (29) have cited cases of the direct transmission of chicken diphtheria to man. In 1884, Chicoli produced diphtheria in fowls by the inoculation of false membranes from man, and observed at the same time diphtheria among children who frequent places used by fowls affected by the disease. Emmerich (30) has also established in his experiments that pigeon diphtheria could be transmitted to man, and *vice versa*. Bermont (31) observed at Bonvilers an epidemic of diphtheria among children affected with scarlatina, who live in places occupied by diphtheritic chickens. Longuet (32) saw the disease in a soldier who attended chickens which were infected. Tissier and Longuet (33) thought that in 40 per cent of the cases infected, manure was the cause of the spread of the disease. This idea was supported by Chauveau (34), who considered the two diseases identical. Barbier (35) frequently saw diphtheria among fowls which lived beside isolated buildings for diphtheria patients, and stated the case of a woman, 67 years of age, who was attacked by diphtheria after having disinfected a poultry building infected with the disease.

Debrie (36) reported an interesting case: Some soldiers affected with diphtheria were admitted to the hospital at Sebdou. Immediately the fowls, which were looked after by a hospital attendant, presented symptoms of diphtheria, and the owner and a soldier who cared for them was also infected. Cole (37) and Schrevens (38) also reported cases of the transmission of chicken diphtheria to children.

Such are the incidents given and statements made by those who believe in the identity of the two diseases, and against them there are almost as many made by the dualists, or those who believe the two diseases entirely distinct.

Thus, a pupil of Trasbot's (39) swallowed the false membranes of several hens without contracting diphtheria. Megnin (40) has never observed diphtheria among those who took care of diphtheritic birds. Rivolta (41), discussing the observations of Gerhardt, said that the false membranes of fowls were not diphtheritic, but croupous, and that if the epidemic observed among the employes at Nesselhausen had been true diphtheria, it would not have remained localised, but would have spread to the environs. It was possible that the infection was transmitted from the fowls to man, but it was not diphtheria, only a slight attack of croup.

Löffler (5), the discoverer of the human diphtheria germ, was never able to produce diphtheria in fowls by the inoculation of the true bacillus, and Colin (43), has arrived at the same results. Nocard (44) denied the identity of the two diseases, and St. Yves Menard (45) considered the false membranes of fowls quite different from those of man, and had never seen cases of the transmission of the disease in the "Jardin d' acclimatation of Paris." Straus had never observed cases of diphtheria among those who took care of diphtheritic pigeons. Gratia and Lienaux (46) had one favorable result from the administration of diphtheritic antitoxin to a diphtheritic chicken; but in all other cases the results were negative. These authors concluded that the inefficacy of this remedy for poultry was an argument against the identity of the avian and human forms of diphtheria.

It is impossible to bring into agreement all the data furnished by the above experimenters, unless we admit that the diphtheria is produced in the birds by several different species of bacteria, and perhaps coccidia, or that under the name of diphtheria there has been, as Galli-Valerio (47) remarks, "a lamentable confusion of different affections." We know that infectious pseudo-membranous inflammation of mucous membranes may be caused by the *Streptococcus pyogenes*, *Staphylococcus pyogenes*, and other bacteria, and many of these disorders caused by these organs resemble very closely the local and general phenomena of diphtheria caused by the Klebs-Löffler bacillus; and, if such is the case in the human subject, it is more than probable it is also true of birds.

Seeing, therefore, that the research upon the etiology of the diphtheritic affections of birds was so incomplete and so controversial we have given this subject considerable attention, the work extending over a period of the last four years, and involving observations and inoculations of some 300 fowls.

The publicity given to Stevenson's paper in the Agricultural Press led us first to examine the effect of human diphtheria antitoxin on birds suffering from diphtheria and having well-characterized pseudo-membranes. The antitoxin was injected subcutaneously; and after the operation, the birds were examined every day for a week, and then at longer intervals. Many were kept under observation for several months.

A series of thirty birds were inoculated with fresh serum obtained from well-recognized sources (Parke Davis and Mulford). The dose varied from 250-550 units, either given at one time or at intervals. Of the thirty birds so treated, but two showed some signs of improvement following immediately after the injection; but three days after they were again badly diseased. The remaining twenty-eight either showed no improvement, or even became worse after the treatment. In ten cases, the disease persisted for three months, when the birds were killed and examined.

A series of eight birds were also treated with fresh horse serum in doses of from 2 to 5 c. c. Two hens treated with the larger dose died twenty-four hours after injection, a proof of the toxic nature of some horse sera, a fact which Cobbett has already pointed out. Two other birds showed some signs of improvement after treatment with smaller doses, and our poultry manager reported to me after trying the fresh horse serum on a number of birds which were diseased, that in some cases he had seen partial cures, but it was not specific, and did not seem of much use.

The results of these experiments, therefore, show that:

1. Diphtheria antitoxin inoculated into birds suffering from fowl diphtheria, and presenting pseudo-membranes, produced no practical or permanent results.

2. Were the germs of human and avian diphtheria the same, this antitoxin certainly must have influenced the fowl diphtheria, since it is the best remedy known for human diphtheria.

3. Ordinary horse serum showed better results than antidiphtheritic serum, but it could not be regarded as a specific.

The third series of experiments was made on five healthy fowls which were inoculated with human diphtheria bacilli, which were obtained from two different sources and isolated on Löffler's serum:

Fowl 1. Inoculated by scratching throat and rubbing in a twenty-four hour old culture of the Klebs-Löffler bacillus. No membrane formed. Bird remained healthy.

Fowl 2. Same procedure, but bacillus from another source. No membrane formed.

Fowl 3. Inoculated subcutaneously with the half of a serum culture of the K.-L. bacillus. No results.

Fowl 4. Same procedure as 3, but bacillus from another source. No ill effects.

Fowl 5. Inoculated in sub-mucosa with about half a c. c. of a K.-L. culture. No membrane formed. Bird remained healthy.

Two guinea pigs, inoculated with the same culture, died within thirty-six hours with typical symptoms of diphtheria.

The results of these experiments, therefore, show that the Klebs-Löffler bacillus is not pathogenic for hens.

We may mention in this connection that we have repeatedly tried to infect rabbits and guinea pigs by the inoculation of false membranes taken from diseased hens, but with little success. We have also kept rabbits and guinea pigs confined in cages with diseased hens, but these animals have never taken the disease. Stevenson stated that in his experiments when rabbits were fed from the same vessel as a roupy bird, they became infected with the disease and died as a result; but experiments conducted here have entirely failed to corroborate this statement. Not only did rabbits live for weeks in coups with roupy birds, without becoming affected, but a doe brought up a litter in a coop in which were kept the worst procurable cases of fowl diphtheria; and these rabbits, though drinking from a vessel used by diseased birds (which was uncleaned for weeks), and eating hay and roots which were in constant contact with the birds, never showed any symptoms of ill-health. If the disease in the fowls had been caused by the Klebs-Löffler bacillus, it would certainly have been pathogenic for both the rabbits and the guinea pigs, as we know these animals are used for standardizing the diphtheria antitoxin.

We have made cultures from over two hundred fowls which died from diphtheria, or were killed in certain stages of the disease, but we have not met with the Klebs-Löffler bacillus in any of them, and, consequently, we cannot believe in the identity of the human and avian disease.

We also find that the structure of the diphtheritic membrane of men and fowls is different. In the former, it consists of necrosed epithelial cells, and inflammation of the connective tissue, with an abundant fibrinous exudation. If the membrane is formed in the trachea instead of the pharynx, the epithelial cells become shed, and the membrane consists almost entirely of fibrin, with leucocytes, the former arranged in laminæ, but varying in density. The diphtheria bacilli are found most frequently in the spaces between the fibrin, and are usually most numerous in the superficial part of the membrane. The bacteria stain very well by Gram's method.

The membrane in fowls, however, consists almost entirely of pus cells, some granular masses, débris of the epithelial cells, especially the swollen nuclei of these, and bacteria; and micro-organisms which stain by Gram's method are seldom found in it. This year, we have succeeded in isolating certain bacteria from diseased birds, and have produced the typical disease in healthy birds by the injection of these bacteria.

Our final results and successes were obtained by using pigeons in order to exalt the virulence of the causal organisms. Chronic diseases, of which we have an excellent example in fowl diphtheria, are notoriously hard to produce by the inoculation of healthy animals; because, in most cases of sickness, there must be not only the causal organism, but a lowering of the vital forces; and, to get over this difficulty, we used pigeons (which are easily affected) to increase the virulence of the causal organism and thereby assist in the infection of hens. In this way, we produced roup in hens at pleasure by inoculation of the roup bacillus taken from pigeons.

The roup bacillus tends to penetrate the deeper layers of the mucous membranes or submucous tissues. Hence, cultures made from the swabs taken from false membranes very rarely contain the roup bacillus, because the bacilli are retained in the depths of the tissue.

The roup bacillus is especially difficult to isolate in cases in which the bird has had the disease for a long time, as the tumors and false membranes contain very many other kinds of bacteria in large numbers. In our experiments, even when roup was produced in healthy fowls by the inoculation of pure cultures of the roup bacillus, the mucous discharge from the beginning contained many different kinds of bacteria.

The roup germs seem capable of remaining in a sort of dormant condition in the depths of the tissues for a very long time, so long that the fowls sometimes appear convalescent; but, suddenly, when the constitution is weakened by a cold or other causes, the roup germs become active, and the diphtheria symptoms reappear.

We have also found that roup, with all its varying symptoms, can be produced also by the inoculation of healthy hens with the well-known *Bacillus pyocyaneus*, or green pus germ, which we have frequently isolated from roupy birds. Hence, it would seem that roup is simply a complex suppurative process, but different from ordinary forms of suppuration. The pus in fowls appears in the form of a half, or entirely solid, cheese-like, yellowish-white mass, without any tendency to become soft, liquid, or to perforate the surrounding skin.

This may be proved by the injection of sterile turpentine oil into the eyelids, which gives rise to inflammation, and the formation of a solid, cheese-like matter in the depths of the tumor.

Therefore, the cheesy masses must be regarded as pus.

The evidence adduced in the preceding paragraphs of this paper, together with the isolation of the causal organisms (the roup bacillus and the *B. pyocyaneus*), and the production of typical fowl diphtheria by the inoculation of both of these bacteria into healthy birds, have, we think, shown the unsoundness of the theory of those who believe in the identity of human and avian diphtheria.

We are unable to state the possible danger of the roup bacillus to man, but we know that the *B. pyocyaneus* produces abscesses, and occasionally gives rise to an epidemic of blood poisoning, as Williams and Cameron have shown; but neither of these bacteria has the same pathogenic power as human diptheria.

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DEMONSTRATION OF A METHOD FOR CAPPING BOTTLES CONTAINING CERTIFIED MILK.

By E. H. WILSON and R. B. F. RANDOLPH, Brooklyn, N. Y.

The "Common Sense" bottle must be used.

The official cap of the Milk Commission is to be placed in the neck of the bottle next to the milk, face up.

Above this is to be placed another cap bearing the date of bottling. This cap should be inserted face down.

On top of the second cap melted paraffin sufficient to entirely cover it should be poured.

The official cap bears the date of the expiration of the dealer's right to use it. Before this date the milk is re-examined.

The caps furnished by the Commission are sterile when issued to the dealer. Packages containing them should not be opened until the caps are needed, and the under side of the cap should not be touched with the fingers. The hands of the bottler should be thoroughly washed before inserting the caps.

CONSTITUTION

OF THE

AMERICAN PUBLIC HEALTH ASSOCIATION.

TITLE.

I. This Association shall be called "THE AMERICAN PUBLIC HEALTH ASSOCIATION."

OBJECTS.

II. The objects of this Association shall be the advancement of sanitary science, and the promotion of organizations and the measures for the practical application of public hygiene.

MEMBERS.

III. The members of this Association shall be known as Active and Associate. The Executive Committee shall determine for which class a candidate shall be proposed. The *Active* members shall constitute the permanent body of the Association, subject to the provisions of the constitution as to continuance in membership. They shall be selected with special reference to their acknowledged interest in, or devotion to, sanitary studies and allied science, and to the practical application of the same. The *Associate* members shall be elected with special reference to their general interest only in sanitary science, and shall have all the privileges and publications of the Association, but shall not be entitled to vote.

Delegates from national, state, provincial, and municipal boards of health, organized sanitary associations, and the army, navy, and marine hospital service, shall be entitled to be enrolled as active members upon presentation of their credentials to the Executive Committee. Members, not delegates from such bodies, shall be elected as follows:

Each candidate for admission shall first be proposed to the Executive Committee, in writing (which may be done at any time), with a statement of the business or profession and special qualifications of the person so proposed. On recommendation of a majority of the committee, and on receiving a vote of two-thirds of the members present at the regular meeting, the candidate shall be declared duly elected a member of the Association. The annual fee of membership in either class shall be five dollars.

OFFICERS.

IV. The officers shall be a President, a First and a Second Vice-President, a Secretary, and a Treasurer, and the officers of the various sections.

All the officers shall be elected by ballot, annually, except the Secretary, who shall be elected for a term of three years.

PRESIDING OFFICER.

V. The President, or in his absence one of the Vice-Presidents, or in their absence a chairman *pro tempore*, shall preside at all meetings of the Association. He shall preserve order, and shall decide all questions of order, subject to appeal to the Association. He shall also appoint all committees authorized by the Association, unless otherwise specially ordered.

SECRETARY.

VI. The Secretary shall have charge of the correspondence and records of the Association; and he shall also perform the duties of Librarian. He, together with the presiding officer, shall certify all acts of the Association. He shall, under the direction of the Executive Committee, give due notice of the time and place of all meetings of the Association, and attend the same. He shall keep fair and accurate records of all the proceedings and orders of the Association; and shall give notice to the several officers, and to the Executive and other committees, of all votes, orders, resolves, and proceedings of the Association, affecting them or appertaining to their respective duties.

TREASURER.

VII. The Treasurer shall collect and take charge of the funds and securities of the Association. Out of these funds he shall pay such sums only as may be ordered by the Association, or by the Executive Committee. He shall keep a true account of his receipts and payments, and at each annual meeting render the same to the Association, when a committee shall be appointed to audit his accounts. If, from the annual report of the Treasurer, there shall appear to be a balance against the treasury, no appropriation of money shall be made for any object but the necessary current expenses of the Association, until such balance shall be paid.

STANDING COMMITTEES.

VIII. There shall be the following standing committees: (1) The Executive Committee, (2) the Advisory Council, (3) the Committee on Publication.

EXECUTIVE COMMITTEE.

IX. The Executive Committee shall consist (1) of the President, First Vice-President, Second Vice-President, Secretary, Treasurer, and Chairmen of the various sections; (2) of six active members of whom three shall be elected annually by ballot to serve two years, and who shall be ineligible to reëlection for a second successive term; and (3) of the ex-presidents of the Association.

It shall be the duty of the Executive Committee to consider and recommend plans for promoting the objects of the Association, to authorize the disbursement and expenditure of unappropriated moneys in the treasury for the payment of current expenses; to consider all applications for membership, and at the regular meetings, report the names of such candidates as a majority shall approve; and, generally, to superintend the interests of the Association and execute all such duties as may, from time to time, be committed to them by the Association. At least one month preceding the annual meeting of the Association, the Executive Committee shall cause to be issued to members a notice of such meeting, and they are authorized to publish the same in medical, scientific, and other periodicals, but without expense to the Association; and such notice shall contain the order of business to be followed at said meeting, and, briefly, the subjects to be presented and the special points of discussion.

ADVISORY COUNCIL.

X. The Advisory Council shall consist of one member from each state, territory, and district, the army, navy, and marine hospital service, the Dominion of Canada, and each of the Provinces, who shall be appointed by the President on the last day of each session, and who, besides acting as nominating committee of officers for the ensuing year, to be announced at such time as the Executive Committee may appoint, shall consider such questions and make such recommendations to the Association as shall best secure the objects of the Association. They shall at their first meeting elect from their own number a Secretary, whose record of their proceedings shall be made part of the records of the Association.

COMMITTEE ON PUBLICATION.

XI. The Committee on Publication shall consist of a Chairman, the Secretary of the Association, the Treasurer of the Association, and a member of each section of the Association, with the President of the Association, *ex officio;* who shall contract for, arrange and publish, under authority of the Executive Committee, the proceedings of the Association, including such papers as have been examined and approved by the Executive Committee, or which have been submitted to them by the latter for their discretionary action.

REPORTS AND PAPERS.

XII. All committees preparing reports to be presented to the Association must submit the same to the Publication Committee not later than thirty days before the succeeding annual meeting of the Association; and all papers other than committee reports shall be in the hands of the Secretary of the Association not less than twenty days before.

MEETINGS.

XIII. The time and place of each annual meeting shall be fixed at the preceding annual meeting, but may be changed by the Executive Committee for reasons that shall be specified in the announcement of the meeting. Special meetings may be called at any time or place, by concurrence of two-thirds of the Executive Committee. There shall be no election of officers, or change of by-laws, or appropriation of money to exceed the amount at that time in the treasury, at such special meeting, except by a vote of a majority of all the members of the Association. Whenever a special meeting is to be held, at least one month's notice shall, if possible, be given by circular to all the members, together with the order of business.

QUORUM.

XIV. At the annual meeting nine members shall constitute a quorum for the election of officers, a change of the constitution, the election of members, and the appropriation of moneys.

ORDER OF BUSINESS.

XV. The order of business at all meetings of the Association shall be fixed by the Executive Committee, and such order must be completed before any other business is introduced, except such order of business is suspended by a vote of four-fifths present.

ALTERATION OF CONSTITUTION.

XVI. No alteration in the constitution of the Association shall be made except at an annual meeting, or unless such alteration shall have been proposed at a previous meeting, and entered on the minutes with the name of the members proposing the same, and shall be adopted by a vote of two-thirds of the members present.

XVII. Members of the Association may be authorized to form themselves into sections by vote of the Association at any annual meeting; such sections shall have power to nominate their officers to be elected by the Association, and to make by-laws which shall go into effect when approved by the Association.

BY-LAWS OF THE EXECUTIVE COMMITTEE.

QUORUM.

1. Five members shall constitute a quorum for the transaction of such business as may come before the committee.

MEMBERS RESTRICTED.

2. No elective member of the Executive Committee shall be at the same time a member of the Advisory Council, if there is another member of the Association from his state or service.

PARLIAMENTARY USAGE.

3. Cushing's Law and Practice of Legislative Assembles shall be the guide of parliamentary practice until otherwise ordered.

PAPERS.

4. Papers to be presented at a general session of the Association shall be in the hands of the Secretary of the Association not less than twenty days before the coming meeting, and to be printed in the transactions in full or in abstract at the discretion of the Publication Committee.

5. All papers accepted by the Association, whether read in full, by abstract, by title, or filed, shall be delivered to the Secretary as soon as thus disposed of, as the exclusive property of the Association. Any paper presented to this Association and accepted by it shall be refused publication in the transactions of the Association, if it be published, in whole or in part, by permission or assent of its author, in any manner

prior to the publication of the volume of transactions, unless written consent is obtained from the Publication Committee.

6. Day papers shall be limited to twenty minutes, and evening papers to thirty minutes each.

DISCUSSION OF PAPERS.

7. After the leading papers on each subject, as indicated by the Executive Committee, have been read, discussion shall follow, and be confined strictly to the subject of these papers; and each speaker shall be limited to ten minutes, and shall not speak a second time until after every other member who desires to be heard, and then only for five minutes, except by unanimous consent.

8. The Chair shall notify gentlemen who desire to speak to send up their names, and they shall be called on in the order sent up; and he may, at his discretion, limit the time of speaking to five instead of ten minutes, if in his judgment it may become necessary to do so in order to allow each one on the list an opportunity to be heard.

PUBLICATION COMMITTEE.

9. The Publication Committee shall consist of a Chairman, the Secretary of the Association, the Treasurer of the Association, and a member of each section of the Association, with the President of the Association *ex officio*.

10. Unless otherwise ordered by the Association, such committee is hereby instructed to keep the size of the annual transactions within the limits of four hundred pages; and to this end to conform to the several following rules:

a. Make summaries of such local and other addresses as are not of permanent scientific value.

b. The Chairman of the several Standing Committees shall be notified by the Secretary that the reports to be presented by them shall include any material prepared by the several members of their committees and must be in the hands of the Publication Committee not later than thirty days before the succeeding annual meeting for preliminary printing.

c. Such reports shall be printed for distribution to the members on registering at the coming meeting; but no report shall be printed unless in the hands of the Publication Committee, as specified above.

11. The Publication Committee shall procure a copyright on the transactions in the name of the Association, and the committee shall have full charge of the publication of the transactions.

APPLICATION FOR MEMBERSHIP.

12. All applications for membership must be made upon the application blank of the Association.

13. Persons not members, having prepared papers to be presented at the meetings of the Association, shall be proposed for membership at the first business meeting of the Association.

EXPENDITURES.

14. All bills connected with the publication of the transactions shall, upon the approval of the Chairman of the Publication Committee and the Secretary, be signed by the President of the Association, and paid by check of the Treasurer directly to the party concerned; and the President shall not approve any bill, relating either to publishing or printing, without the approval first of the chairman of the committee in charge thereof.

15. Bills for current expenses shall be first approved by the Secretary, then sent to the President, and on his approval they shall be paid by check of the Treasurer directly to the parties interested.

16. The actual and necessary traveling expenses of the Secretary and Treasurer to the annual meeting of the Association, and to one meeting of the Executive Committee, shall be classed as current expenses.

RESOLUTIONS.

17. All resolutions presented to the Association shall be sent to the Chair in writing, and referred to a committee without discussion.

ARREARAGES.

18. The arrearages of all members remitting their dues for two years shall be cancelled up to the date of the last payment, but they shall be entitled to the transactions of the Association only for the years for which they have actually paid.

AUDITING COMMITTEE.

19. An Auditing Committee shall be appointed by the Chair to audit the accounts of the Treasurer, and report upon the same.

OFFICERS AND COMMITTEES

OF THE

AMERICAN PUBLIC HEALTH ASSOCIATION.

OFFICERS, 1902-1903.

President	Dr. Walter Wyman, Washington, D. C.
First Vice-President	Dr. C. P. Wilkinson, New Orleans, La.
Second Vice-President	Dr. John L. Leal, Paterson, N. J.
Secretary	Dr. Charles O. Probst, Columbus, Ohio.
Treasurer	Dr. Frank W. Wright, New Haven, Conn.

PRESIDENTS OF THE ASSOCIATION.

Stephen Smith, M. D	1872, '73 '74
*Joseph M. Toner, M. D	1875
*Edwin M. Snow, M. D	1876
*John H. Rauch, M. D	1877
*Elisha Harris, M. D	1878
*James L. Cabell, M. D	1879
John S. Billings, M. D	1880
*Charles B. White, M. D	1881
*Robert C. Kedzie, M. D	1882
*Ezra M. Hunt, M. D	1883
*Albert L. Gihon, M. D	1884
*James E. Reeves, M. D	1885
Henry P. Walcott, M. D	1886
George M. Sternberg, M. D	1887
Charles N. Hewitt, M. D	1888
*Hosmer A. Johnson, M. D	1889
Henry B. Baker, M. D	1890
Frederick Montizambert, M. D	1891
Felix Formento, M. D	1892
Samuel H. Durgin, M. D	1893
Emmanuel P. Lachapelle, M. D	1894
William Bailey, M. D	1895
Eduardo Licéaga, M. D	1896
*Henry B. Horlbeck, M. D	1897
Charles A. Lindsley, M. D	1898
*George H. Rohé, M. D	1899
Henry Mitchell, M. D	1899
Peter H. Bryce, M. D	1900
Benjamin Lee, M. D	1901
Henry D. Holton, M. D	1902

*Deceased.

STANDING COMMITTEES.

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(The officers and Ex-Presidents are *ex-officio* members of the Executive Committee.)

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Dr. John S. Fulton	Baltimore, Md.
Dr. José Ramirez	Mexico, Mex.
Dr. John A. Amyot	Toronto, Ont.

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Dr. Josiah Hartzell	Canton, Ohio.
Dr. C. O. Probst	Columbus. Ohio.
Dr. Frank W. Wright	New Haven, Conn.
Dr. Herbert D. Pease	Albany, N. Y.

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Missouri	Dr. D. T. Powell, *Thayer.*
Nebraska	
New Hampshire	Dr. G. P. Conn, *Concord.*
New Jersey	Dr. John Lainy Leal, *Paterson.*
New Mexico	
New York	Dr. Joseph H. Raymond, *Brooklyn.*
North Carolina	Dr. R. H. Lewis, *Raleigh.*

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Ohio	Dr. W. C. Chapman, *Toledo.*
Oklahoma Territory	
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Porto Rico	Dr. William Fawcett Smith, *San Juan.*
Rhode Island	Dr. C. V. Chapin, *Providence.*
South Carolina	Dr. Charles M. Rees, *Charleston.*
South Dakota	
Tennessee	Dr. Marcus Haase, *Memphis.*
Texas	Dr. J. H. Smart, *Dallas.*
Vermont	Dr. C. S. Caverly, *Rutland.*
Virginia	
Washington	Dr. James B. Eagleson, *Seattle.*
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District of Columbia	Dr. William C. Woodward, *Washington, D. C.*
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U. S. P. H. and M. H. Service	Dr. Henry R. Carter, *Baltimore, Md.*
U. S. Navy	Dr. John W. Ross, *Washington, D. C.*

CANADA:

Dominion of Canada	Dr. Frederick Montizambert, *Ottawa.*
British Columbia	Dr. J. C. Fagan, *Victoria, B. C.*
Province of Ontario	Dr. J. A. Amyot, *Toronto.*
Province of Quebec	Dr. E. P. Lachapelle, *Montreal.*
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New Brunswick	Dr. E. Bayard Fisher, *Fredericton.*
Nova Scotia	

CUBA:

Republic of Cuba	Dr. Carlos Finlay, *Havana.*

MEXICO:

Aguas Calientes	Dr. José Ramirez, *Mexico, D. F.*
Campeche	Dr. Domingo Orvanaños, *Mexico, D. F.*
Colima	Dr. Luis E. Ruiz, *Mexico, D. F.*
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Chihuahua	Dr. Miguel Marquez, *Chihuahua.*
Chiapas	
Durango	Dr. José L. Gomez, *Mexico, D. F.*
Guanajuato	Dr. Jésus Chico, *Guanajuato City.*
Guerrero	
Hidalgo	Dr. Ramon Prado, *Mexico, D. F.*
Jalisco	Dr. Jésus Gonzales Ureña, *Mexico, D. F.*
Michoacan	Dr. Francisco Bernaldez, *Mexico, D. F.*
Morelos	Dr. Fernando Lopez, *Mexico, D. F.*
Mexico	Dr. Ignacio Guzman, *Toluca.*
Nuevo Leon	Dr. Pedro Noriega, *Monterey.*
Oaxaca	Dr. Adelberto Carriedo, *Oaxaca.*
Puebla	Dr. J. Joaquin Urrutia, *Puebla.*
Queretaro	Dr. Augustin Ruiz Ollogui, *San Juan del Rio Qro.*
San Luis Potosi	Dr. Jésus E. Monjaras, *San Luis Potosi.*

Sonora	Dr. Angel Gaviño, *Mexico, D. F.*
Sinaloa	Dr. Narciso Del Rio, *Veracruz.*
Tabasco	Dr. Gonzales Fabela, *Mexico, D. F.*
Tamaulipas	Dr. Antonio Matienzo, *Tampico.*
Tlaxcala	Dr. José Gayon, *Mexico, D. F.*
Veracruz	Dr. Manuel Iglesias, *Veracruz.*
Yucatan	Dr. Augustin Chacon, *Mexico, D. F.*
Zacatecas	Dr. Juan Breña, *Zacatecas.*
Territorio de la Baja California	
Territorio de Tepic	
Distrito Federal	Dr. Eduardo Licéaga, *Mexico, D. F.*

SPECIAL COMMITTEES.

PURIFICATION OF WATER SUPPLIES.

George W. Fuller	New York, N. Y.
Marshall Leighton	Chicago, Ill.
Robert S. Weston, C. E.	Boston, Mass.
H. W. Clark	Boston, Mass.

DISPOSAL OF INDUSTRIAL WASTES.

Dr. G. T. Swarts	Providence, R. I.
Allen Hazen, C. E.	New York, N. Y.
Prof. L. P. Kinnicutt	Worcester, Mass.

PURIFICATION AND DISPOSAL OF SEWAGE.

Geo. H. Benzenberg, C. E.	Milwaukee, Wis.
J. W. Alvord, C. E.	Chicago, Ill.
J. P. Force, C. E.	Columbus, Ohio.

DISPOSAL OF GARBAGE.

Rudolph Hering, C. E.	New York, N. Y.
Dr. Elzear Pelletier	Montreal, P. Q.
Dr. W. C. Woodward	Washington, D. C.
Dr. José L. Gomez	Mexico, Mex.
Prof. O. H. Landreth	Schenectady, N. Y.
Dr. Wm. C. Teichmann	St. Louis, Mo.
G. H. Benzenberg, C. E.	Milwaukee, Wis.

ANIMAL DISEASE AND FOOD.

D. E. Salmon, D. V. M.	Washington, D. C.
Dr. M. P. Ravenel	Philadelphia, Pa.
Prof. José L. Gomez	Mexico, Mex.
Dr. J. G. Rutherford	Ottawa, Ont.
Dr. I. A. Watson	Concord, N. H.
Dr. V. A. Moore	Ithaca, N. Y.
Dr. H. L. Russell	Madison, Wis.

CAR SANITATION.

Dr. G. P. Conn	Concord, N. H.
Dr. John N. Hurty	Indianapolis, Ind.
Dr. Chas. P. Dudley	Altoona, Pa.
Dr. R. H. Lewis	Raleigh, N. C.
Dr. W. G. Bissell	Buffalo, N. Y.
Dr. Francisco Bernaldez	Mexico, Mex.

STEAMBOAT AND STEAMSHIP SANITATION.

Dr. Peter H. Bryce	Toronto, Ont.
Dr. J. N. McCormack	Bowling Green, Ky.
Dr. Walter D. Greene	Buffalo, N. Y.
Dr. Heber Jones	Memphis, Tenn.
Dr. Manuel Iglesias	Veracruz, Mex.

ETIOLOGY OF YELLOW FEVER.

Dr. John W. Ross, U. S. Navy	Washington, D. C.
Dr. Eduardo Licéaga	Mexico, Mex.
Dr. Juan Guiteras	Havana, Cuba.
Dr. Henry R. Carter, P. H. and M. H. S	Washington, D. C.
Dr. Paul E. Archinard	New Orleans, La.

TUBERCULOSIS.

Dr. William C. Gorgas	Washington, D. C.
Dr. M. P. Ravenel	Philadelphia, Pa.
Dr. James A. Burroughs	Asheville, N. C.

PLAGUE.

Dr. Alvah H. Doty	New York, N. Y.
Dr. J. J. Kinyoun	Washington, D. C.
Dr. C. J. Fagan	Victoria, B. C.
Dr. M. J. Rosenau, P. H. and M. H. S	Washington, D. C.

DEMOGRAPHY AND STATISTICS IN THEIR SANITARY RELATION.

Wm. A. King	Washington, D. C.
Dr. Cressy L. Wilbur	Lansing, Mich.
Dr. J. S. Fulton	Baltimore, Md.
Dr. H. M. Bracken	St. Paul, Minn.
Dr. Jèsus E. Monjaràs	Mexico D. F.
F. L. Hoffman	Newark, N. J.

CAUSE AND PREVENTION OF INFECTIOUS DISEASES.

Dr. A. R. Reynolds	Chicago, Ill.
Dr. C. S. Caverly	Rutland, Vt.
Dr. J. H. Raymond	Brooklyn, N. Y.
Dr. Francisco Hurtado	Mexico, Mex.

INFECTIOUS PERIOD OF COMMUNICABLE DISEASES.

Dr. S. H. Durgin	Boston, Mass.

PUBLIC HEALTH LEGISLATION.

Dr. C. O. Probst	Columbus, Ohio.
Dr. José Ramirez	Mexico, Mex.
Dr. E. P. Lachapelle	Montreal, P. Q.

CAUSE AND PREVENTION OF INFANT MORTALITY.

Dr. Henry Mitchell	Asbury Park, N. J.
Dr. J. C. Shrader	Iowa City, Iowa.
Dr. J. A. Egan	Springfield, Ill.
Dr. J. Joaquin Urrutia	Puebla, Mex.

DISINFECTANTS AND DISINFECTION.

Prof. F. C. Robinson	Brunswick, Me.
Dr. Hibbert W. Hill	Boston, Mass.
Dr. H. D. Pease	Albany, N. Y.
Dr. Chas. V. Chapin	Providence, R. I.
Dr. Luis E. Ruiz	Mexico, Mex.

PRODUCTION AND FREE DISTRIBUTION OF VACCINE.

Dr. Henry P. Walcott	Cambridge, Mass.
Dr. F. Montizambert	Ottawa, Ont.
Dr. Francisco Bernaldez	Mexico, Mex.

TEACHING OF HYGIENE AND GRANTING OF DIPLOMA OF DOCTOR OF PUBLIC HEALTH.

Dr. Benjamin Lee	Philadelphia, Pa.
Dr. F. F. Wesbrook	Minneapolis, Minn.
Dr. T. A. Starkey	Montreal, P. Q.
Prof. Wm. T. Sedgwick	Boston, Mass.
Dr. Eleanor Parry	South Hadley, Mass.
Dr. Jèsus Gonzales Urena	Mexico, Mex.

SANITARY AID SOCIETIES.

Dr. George W. Goler	Rochester, N. Y.
Miss Hester McClung	Indianapolis, Ind.
Mr. Henry Lomb	Rochester, N. Y.
Dr. Helen C. Putnam	Providence, R. I.

CANTEEN IN ARMIES.

Dr. George M. Kober	Washington, D. C.
Dr. Jésus E. Monjarás	Mexico, D. F.
Dr. F. C. Harrison	Guelph, Ont.

SECTION OF BACTERIOLOGY AND CHEMISTRY.

OFFICERS AND COMMITTEES, 1902-1903.

Chairman	Dr. H. L. Russell, Madison, Wis.
Vice-Chairman	Dr. V. A. Moore, Ithaca, N. Y.
Secretary	Mr. G. C. Whipple, Brooklyn, N. Y.
Recorder	Dr. H. D. Pease, Albany, N. Y.

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Dr. W. H. Park	New York, N. Y.
Dr. M. J. Rosenau	Washington, D. C.
Mr. E. G. Horton	Columbus, Ohio.
Dr. Angel Gaviño	Mexico City, Mexico.
Dr. F. F. Wesbrook	Minneapolis, Minn.

STANDARD METHODS OF WATER ANALYSIS.

G. W. Fuller	New York, N. Y.
H. L. Russell	Madison, Wis.
R. S. Weston	Boston, Mass.
J. W. Ellms	Cincinnati, Ohio.
G. C. Whipple	Brooklyn, N. Y.
H. W. Clark	Boston, Mass.
E. O. Jordon	Chicago, Ill.

THE VALUE OF A BACTERIAL STANDARD FOR MUNICIPAL MILK SUPPLIES.

W. H. Park	New York, N. Y.
Ezra Wilson	Brooklyn, N. Y.
R. G. Freeman	New York, N. Y.
H. L. Russell	Madison, Wis.
F. C. Harrison	Guelph, Ont.

ON DIAGNOSTIC PROCEDURES.

F. F. Wesbrook	Minneapolis, Minn.
W. H. Park	New York, N. Y.
H. W. Hill	Boston, Mass.
F. P. Denny	Brookline, Mass.
A. C. Abbott	Philadelphia, Pa.

THE SIGNIFICANCE OF BACILLUS COLI IN WATER SUPPLIES.

S. C. Prescott	Boston, Mass.
S. De. M. Gage	Boston, Mass.
B. H. Stone	Burlington, Vt.
E. G. Horton	Columbus, O.
W. G. Bissell	Buffalo, N. Y.

ON BACILLUS TUBERCULOSIS IN MAN AND ANIMALS.

M. P. Ravenel	Philadelphia, Pa.
V. A. Moore	Ithaca, N. Y.
Theobald Smith	Boston, Mass.
E. A. De Schweinitz	Washington, D. C.

ON LABORATORY TESTS OF THE EFFICIENCY OF WATER FILTERS.

G. T. Swarts	Providence, R. I.
A. Gehrmann	Chicago, Ill.
R. S. Weston	Boston, Mass.
B. H. Buxton	New York, N. Y.
L. P. Kinnicutt	Worcester, Mass.

THE ACTION OF FORMALDEHYDE AS A DISINFECTANT.

H. W. Hill	Boston, Mass.
J. S. Fulton	Baltimore, Md.
E. G. Horton	Columbus, O.
M. P. Ravenel	Philadelphia, Pa.
W. H. Park	New York, N. Y.

ON ANTITOXIC AND IMMUNIZING SERA.

H. D. Pease	Albany, N. Y.
W. H. Park	New York, N. Y.
J. J. Kinyoun	Glenolden, Pa.
Theobald Smith	Boston, Mass.
Joseph McFarland	Philadelphia, Pa.

THE RELATION OF PROTOZOA TO DISEASE.

Theobald Smith	Boston, Mass.
P. E. Archinard	New Orleans, La.
G. E. Beyer	New Orleans, La.
L. B. Wilson	Minneapolis, Minn.
Gonzales Fabela	Mexico City, Mex.

THE VARIETIES AND ACTION OF ANAEROBIC BACTERIA IN SEWAGE.

J. A. Amyot	Toronto, Ont.
C.-E. A. Winslow	Boston, Mass.
L. P. Kinnicutt	Worcester, Mass.

REVISED LIST OF MEMBERS

AMERICAN PUBLIC HEALTH ASSOCIATION.

1902.

This list includes those who have maintained their membership to the present time, counting those joining in 1902. The Secretary should be notified of any errors or omissions.

MEMBERS.

Abbott, Samuel W., M. D.	Wakefield, Mass.	1882
*Abbott, Alex C., M. D.	University of Pennsylvania, Philadelphia, Pa.	1897
Adams, C. B., M. D.	Sac City, Iowa.	1900
Adams, Charles Everett, M. D.	29 W. Broadway, Bangor, Me.	1894
Adams, J. F. A., M. D.	Pittsfield, Mass.	1881
*Addy, G. A. B., M. D.	St. John, N. B.	1899
Adgate, Grace	515 Lexington Ave., New York, N. Y.	1901
Alden, C. H., M. D., U. S. A.	Newtonville, Mass.	1887
Alexander, Edward, M. D.	El Paso, Tex.	1892
Allen, Kenneth, C. E.	Equitable Building, Baltimore, Md.	1899
Allen, Wm. Harvey	University of Pennsylvania, Philadelphia, Pa.	1899
Alvord, J. W., C. E.	127 Hartford Building, Chicago, Ill.	1899
Amherst College Library	Amherst, Mass.	1901
*Amyot, Jno. A., M. D.	30 St. Joseph St., Toronto, Ont.	1900
Anders, H. S., M. D.	1836 Wallace St., Philadelphia, Pa.	1897
Arment, Samuel B., M. D.	Bloomsburg, Pa.	1901
Armitage, F. L., M. D.	Princeton, Minn.	1900
*Badger, Ernest F.	48 Weybosset St., Providence, R. I.	1900
Baggs, A. N., M. D.	Abington, Pa.	1901
Bailey, William, M. D.	Louisville, Ky.	1879
Bailhache, P. H., M. D., P. H. and M. H. S.	Stapleton, N. Y.	1874
Baker, Henry B., M. D.	Lansing, Mich.	1873
*Baldwin, E. R., M. D.	Saranac Lake, N. Y.	1899
Barbour, Frank A., C. E.	Tremont Bldg., Boston, Mass.	1900
Barnes, Henry Jabez, M. D.	429 Beacon St., Boston, Mass.	1896
Bartlett, James W.	Dover, N. H.	1888
Bashore, Harvey B., M. D.	West Fairview, Pa.	1897
Beaudrey, J. A., M. D.	65 St. Denis St., Montreal, P. Q.	1889
Beckwith, D. H., M. D.	94 Dorchester Ave., Cleveland, Ohio.	1884
Beebe, Henry E., M. D.	Sidney, Ohio.	1882
Belknap, Fred R., M. D.	Niles, Mich.	1900
Bell, A. N., M. D.	337 Clinton St., Brooklyn, N. Y.	1872
Bell, Clark	39 Broadway, New York, N. Y.	1901
*Bell, Gordon, M. D.	Winnipeg, Manitoba.	1899
Bence, Geo. W., M. D.	Greencastle, Ind.	1900

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* Also Members of Section of Bacteriology and Chemistry.

Bennitt, Francis M., M. D.	Springfield, Mass.	1901
Benzenberg, Geo. H., C. E.	436 Jefferson St., Milwaukee, Wis.	1900
Bernaldez, Francisco, M. D.	Estampa-Merced 5, Mexico, Mex.	1892
Beshoar, Michael, M. D.	Trinidad, Col.	1895
Bessonetti, B. J. L., M. D.	St. Espriets, P. Q.	1901
*Bevier, Isabel	University of Illinois, Urbana, Ill.	1900
*Bissel, Wm. G., M. D.	143 Elmwood Ave., Buffalo, N. Y.	1900
Bitting, A. Wayne	313 State St., Lafayette, Ind.	1900
Blackwell, Emily, M. D.	53 E. 20th St., New York, N. Y.	1873
Boardman, Wm. Frears	Ottawa, Ont.	1898
Board of Health	Manchester, N. H.	1899
*Bolling, Geo. E.	1257 Belmont St., Brockton, Mass.	1900
Bracken, Henry Martyn, M. D.	St. Paul, Minn.	1895
Brackett, C. F., M. D.	Princeton, N. J.	1899
Brand, Walter W., M. D.	216 Michigan Ave., Toledo, Ohio.	1901
Braunnagel, Julius, M. D.	Box 925, San Antonio, Tex.	1890
Breña, Juan, M. D.	Zacatecas, Mex.	1898
Brewer, Prof. William H., M. D.	New Haven, Conn.	1874
Brown, Joseph O.	Pittsburg, Pa.	1888
Bryce, George	Indianapolis, Ind.	1900
Bryce, Peter H., M. D.	Toronto, Ont.	1883
Buechner, W. L., M. D.	34 E. Federal St., Youngstown, Ohio.	1900
*Burrage, Severance, S. B.	Lafayette, Ind.	1897
Burroughs, Jas. A., M. D.	Asheville, N. C.	1898
*Buxton, Bertram, M. D.	1 West 101st St., New York, N. Y.	1901
Cabana, Louis V., M. D.	Fall River, Mass.	1898
Cairns, R. A., C. E.	Waterbury, Conn.	1901
*Caird, James M., M. D.	52 Powling Ave., Troy, N. Y.	1900
Cameron, Allen, M. D.	Owen Sound, Ont.	1886
Cameron, J. Chalmers, M. D.	941 Dorchester St., Montreal, P. Q.	1894
Carnegie Library	Pittsburg, Pa.	1902
Carriedo, Adelberto, M. D.	Oaxaca, Mex.	1901
Caverly, Charles S., M. D.	Rutland, Vt.	1894
Chaillé, Prof. Stanford E.	P. O. Drawer 261, New Orleans, La.	1874
*Chapin, Jules, M. D.	496½ Rue Rachel, Montreal, P. Q.	1900
Chapman, W. C., M. D.	1708 Madison St., Toledo, Ohio.	1900
Chandler, Prof. C. F.	Columbia College, New York, N. Y.	1872
Chandler, David D.	74 No. 7th St., Newark, N. J.	1896
Chapin, Charles V., M. D.	Providence, R. I.	1886
Charlton, George A., M. D.	McGill University, Montreal, P. Q.	1901
Chase, Herman L., M. D.	Brookline, Mass.	1895
Chase, John C., C. E.	Derry, N. H.	1890
Chausse, Alcide	1539 Notre Dame St., Montreal, P. Q.	1901
Chico, Jésus, M. D.	2 Estrella St., Guanajuato City, Guanajuato, Mex.	1891
Church, Thomas Teasdale, M. D.	70 E. Main St., Salem, Ohio.	1890
*Clark Harry W.	State House, Boston, Mass.	1896
Clark, J. Fred, M. D.	Fairfield, Ia.	1899
Coffey, James C.	Worcester, Mass.	1896
Conn, G. P., M. D.	Concord, N. H.	1875
*Connell, Walter Thomas, M. D.	Kingston, Ont.	1898

* Also Members of Section of Bacteriology and Chemistry.

*Connelly, R. N., M. D.	Newark, N. J.	1898
Conner, P. S., M. D.	Cincinnati, Ohio.	1884
Cook, J. F., M. D.	636 Delaware Ave., Buffalo, N. Y.	1901
Coon, Jno. W., M. D.	City Hall, Milwaukee, Wis.	1900
*Copeland, Wm. R.	Spring Garden Testing Station, Philadelphia, Pa.	1900
*Coplin, William L.	1629 So. Broad St., Philadelphia, Pa.	1897
Corbett, J. F., M. D.	2446 Park Ave., Minneapolis, Minn.	1899
*Coster, W. H.	Center and Craig Sts., Pittsburg, Pa.	1901
Cowing, Hugh A., M. D.	210 High St., Muncie, Ind.	1900
Cox, Edgar, M. D.	13½ W. Mulberry St., Kokomo, Ind.	1900
Craig, G. G., M. D.	Rock Island, Ill.	1880
Crandall, Wm. S.	253 Broadway, New York, N. Y.	1901
Craven, McDonough	263 82d St., Brooklyn, N. Y.	1899
Croes, J. James R., C. E.	68 Broad St., New York, N. Y.	1877
Currier, David M., M. D.	Newport, N. H.	1883
Curtis, George Francis, M. D.	West Newton, Mass.	1897
Curtis, F. C., M. D.	Albany, N. Y.	1883
Dalby, William Thomas, M. D.	Salt Lake City, Utah.	1894
*Davis, Floyd, Ph. D.	Des Moines, Iowa.	1890
Davis, N. S., M. D.	65 Randolph St., Chicago, Ill.	1887
Davis, T. Henry, M. D.	1021 Main St., Richmond, Ind.	1900
de Arellano, Nicolas R., M. D.	Mexico, Mex.	1900
Dedrick, Albert C., M. D.	247 No. Main St., Fall River, Mass.	1900
De Lano, Wm., M. D.	122 Monroe St., Grand Rapids, Mich.	1901
del Moral, Manuel, M. D.	Irapuato,Mex.	1901
*Denney, F. P., M. D.	Brookline, Mass.	1901
Dennis, Laban, M. D.	30 Central St., Newark, N. J.	1877
*de Schweinitz, Emil A., M. D.,	Bureau Animal Industry, Washington, D. C.	1896
Didlake, Madison T., M. D.	Monticello, Ind.	1900
Disbrow, Wm. S., M. D.	Newark, N. J.	1899
Dodds, Wm. T. S., M. D.	18 Ohio St., Indianapolis, Ind.	1900
Dodge, Charles Wright	University of Rochester, Rochester, N. Y.	1901
Dodge, William E., Jr.	11 Cliff St., New York, N. Y.	1874
*Dorset, Marion, M. D.	Bureau Animal Industry, Washington, D. C.	1900
Doty, Alvah H., M. D.	Quarantine, Staten Island, N. Y.	1896
*Drown, Thomas M., M. D.	South Bethlehem, Pa.	1887
Dudley, Charles P., M. D.	Altoona, Pa.	1888
*Dunham, E. K., M. D.	338 26th St., New York, N. Y.	1897
Durgin, Samuel H., M. D.	12 Beacon St., Boston, Mass.	1875
Eagleson, James B., M. D.	512 Burke Building, Seattle, Wash.	1896
Eaton, J. M., M. D.	Carbray, Manitoba.	1901
Egan, J. A., M. D.	Springfield, Ill.	1897
Egbert, Seneca, M. D.	17th and Cherry Sts., Philadelphia, Pa.	1901
Elgin, W. F., M. D.	Glendolen, Pa.	1900
*Ellms, J. W.	East Court and Martin Sts., Cincinnati, Ohio.	1897
Elsner, John, M. D.	Denver, Col.	1887
*Emerson, H. C., M. D.	Springfield, Mass.	1899
*Ernst, Harold C., M. D.	Harvard Medical School, Boston, Mass.	1899

* Also Members of Section of Bacteriology and Chemistry.

Espinosa, Luis, M. D.................................Jalapa, Mex. 1901
Exton, James A., M. D...............75 Beech St., Arlington, N. J. 1897

*Fabelo, Octavino Gonzalez, M. D., 10 Calle de la Violetta, No. 6, Mexico, Mex. 1901
Fagan, C. J., M. D.....................................Victoria, B. C. 1901
Fairfield, Wm. J., M. D............1302 Meridian St., Anderson, Ind. 1900
Fales, Frank L., C. E..................City Hall, Cincinnati, Ohio. 1893
Farnham, Edwin, M. D............................Cambridge, Mass. 1889
Farquhar, A. B., Esq.......................................York, Pa. 1880
Fenton, Thomas H., M. D..Spruce and Juniper Sts., Philadelphia, Pa. 1897
Ferguson, F., M. D................20 W. 38th St., New York, N. Y. 1892
Fisher, E. B., M. D..............................Fredericton, N. B. 1901
Flanned, Frank W.........4020 No. Meridian St., Indianapolis, Ind. 1900
Flick, Lawrence F., M. D............736 Pine St., Philadelphia, Pa. 1890
Florence, John H., M. D...........................Dallas, Texas. 1899
Foote, Charles Jenkins, M. D......26 Elm St., New Haven, Conn. 1895
Force, Jno. P., C. E...............................Columbus, Ohio. 1900
*Ford, W. W., M. D.............McGill University, Montreal, P. Q. 1900
Formento, Felix, M. D..........................New Orleans, La. 1880
*Freeman, R. G., M. D.............478 W. 59th St., New York, N. Y. 1899
Fries, Albert, M. D..................92 Reade St., New York, N. Y. 1897
*Fuller, George W..................170 Broadway, New York, N. Y. 1892
Fulton, Jno., C. E...................................Johnstown, Pa. 1901
Fulton, John S., M. D.................10 South St., Baltimore, Md. 1897
Furniss, Henry W., M. D.............................Bahia, Brazil. 1900

Gable, Isaac C., M. D.......................26 South St., York, Pa. 1897
*Gage, Stephen DeM...........Experiment Station, Lawrence, Mass. 1901
Garciadiego, Salvador, M. D............Guadalajara, Jalisco, Mex. 1892
Gaviño, A., M. D.........................Ratas No. 4, Mexico, Mex. 1891
*Gehrmann, Adolph, M. D................3816 Ellis St., Chicago, Ill. 1895
Gemmill, W. T., M. D.....................................Forest, Ohio. 1900
Gerhard, Wm. Paul, C. E....33 Union Square West, New York, N. Y. 1880
Gerrish, Frederic H., M. D.............................Portland, Me. 1885
*Gilliland, S. H., M. D...............3608 Pine St., Philadelphia, Pa. 1900
Goodridge, E. A., M. D.............63 Jamaica Ave., Flushing, N. Y. 1900
Glover, John F., M. D...............Woodmere St., Evansville, Ind. 1900
Goler, Geo. W., M. D..............127 East Ave., Rochester, N. Y. 1900
Good, J. Louis.........................610 City Hall, Philadelphia, Pa. 1901
Goodwin, R. S., M. D..............................Thomaston, Conn. 1886
Gorgas, Wm. C., M. D., U. S. A..................Washington, D. C. 1901
*Gorham, Fred P.....................................Providence, R. I. 1899
Graves, E. E., M. D....................................Penacook, N. H. 1886
Graves, Prof. Henry S..........Yale University, New Haven, Conn. 1900
Gray, Crosby........................4618 Forbes St., Pittsburg, Pa. 1874
Gray, Samuel M., C. E...........10 Weybosset St., Providence, R. I. 1891
Greenleaf, Chas. R., M. D., U. S. A..............San Francisco, Cal. 1901
Griffith, S. H., M. D...........1308 N. H. Ave., Washington, D. C. 1901
Gutierrez, Manuel, M. D............13 Aduana Vieja, Mexico, Mex. 1891
Guzman, Ignacio, M. D.................................Toluca, Mex. 1900

* Also Members of Section of Bacteriology and Chemistry.

Hagey, Jacob Moyer, M. D.	Mt. Morris, N. Y.	1892
Haigh, Henry A.	47 Alexandria Ave., Detroit, Mich.	1901
Hammer, R. B., M. D.	Greensburg, Pa.	1901
*Harrington, Charles, M. D.	Harvard Med. School, Boston, Mass.	1901
*Harris, Norman McL., M. D.	Baltimore, Md.	1899
Harris, R. W., M. D.	621 Vincennes St., New Albany, Ind.	1900
Harrison, F. C. S., M. D.	Guelph, Ont.	1901
Harrison, George Wood, M. D.	404 Second St., Ashland, Wis.	1896
Hartzell, Josiah, Ph. D.	Canton, Ohio.	1895
Harvard College Library	Cambridge, Mass.	1900
Haskell, W. A., M. D.	Alton, Ill.	1883
Hayes, Franklin W., M. D.	235 N. Penn. St., Indianapolis, Ind.	1900
*Hazen, Allen, C. E.	220 Broadway, New York, N. Y.	1892
*Hektoen, Ludwig, M. D.	Rush Medical College, Chicago, Ill.	1899
Hengerer, William	Buffalo, N. Y.	1896
Hering, Rudolph, C. E.	170 Broadway, New York, N. Y.	1878
Herold, H. C. H., M. D.	Newark, N. J.	1898
Hicks, Charles A., M. D.	Fall River, Mass.	1897
Hill, G. Everett	156 Fifth Ave., New York, N. Y.	1897
*Hill, Hibbert W., M. D.	Sudbury and Hawkins Sts., Boston, Mass.	1896
Hill, John W., C. E.	City Hall, Philadelphia, Pa.	1896
*Hiss, Philip H., Jr., M. D.	Washington Sq. No., New York, N. Y.	1899
*Hodenpyl, Eugene, M. D.	437 W. 59th St., New York, N. Y.	1901
Hohenschuh, W. P.	Iowa City, Ia.	1899
Holmes, A. Mansfield, M. D.	Denver, Col.	1899
Holton, Henry D., M. D.	Brattleboro, Vt.	1875
Hopkins, Henry R., M. D.	433 Franklin St., Buffalo, N. Y.	1894
*Horton, Elmer G., B. S.	Columbus, Ohio.	1897
*Huddleston, J. H., M. D.	126 W. 85th St., New York, N. Y.	1899
Hunt, A. Clark, M. D.	Metuchen, N. J.	1888
Hunt, C. C., M. D.	Dixon, Ill.	1889
Hunt, Wm. T.	204 Market St., Newark, N. J.	1901
Hurty, John N., M. D.	29 East Second St., Indianapolis, Ind.	1896
Irons, Ernest E.	University of Chicago, Chicago, Ill.	1900
Iglesias, Manuel S., M. D.	Veracruz, Mex.	1900
*Jackson, Daniel D.,	Flatbush Ave. and Eastern Parkway, Brooklyn, N. Y.	1899
James, Bushrod W., M. D.	Cor. 18th and Green Sts., Philadelphia, Pa.	1878
Jessop, Roland, M. D.	York, Pa.	1901
Johnson, Chas. B., M. D.	Champaign, Ill.	1897
*Johnson, Geo. A.	302 Main St., Poughkepsie, N. Y.	1900
Jones, C. Hampson, M. D.	211 W. Franklin St., Baltimore, Md.	1898
Jones, D. C., M. D.	Danville, Ill.	1900
Jones, Leander P., M. D.	Greenwich, Conn.	1889
Jones, S. Case, M. D.	21 East Ave., Rochester, N. Y.	1895
*Jordan, Edwin O.	University of Chicago, Chicago, Ill.	1899
Justice, Wm. W.	Germantown, Philadelphia, Pa.	1901
Karsner, Charles W., M. D.	1320 So. Broad St., Philadelphia, Pa.	1897
Kellogg, J. H., M. D.	Battle Creek, Mich.	1878

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Kelso, Wilbur M.	5833 Monroe Ave., Chicago, Ill.	1899
Kendall, W. P., M. D.	Fort Porter, Buffalo, N. Y.	1901
Kendrick, Minnie M.	3507 Baring St., Philadelphia, Pa.	1901
Kennedy, J. F., M. D.	Des Moines, Iowa.	1885
King, William A.	Census Bureau, Washington, D. C.	1901
*Kinnicutt, Leonard P., S. D.	Worcester, Mass.	1899
*Kneass, S. S., M. D.	1510 Walnut St., Philadelphia, Pa.	1899
*Knowles, Morris, C. E.	Bureau of Filtration, Pittsburg, Pa.	1901
Knox, S. P. B., M. D.	912 Anacapa, Santa Barbara, Calif.	1892
Knox, William F., M. D.	McKeesport, Pa.	1887
Kober, George M., M. D.	1600 T St., Washington, D. C.	1896
Kuichling, Emil, C. E.	3 W. 20th St., New York, N. Y.	1882
Lachapelle, E. Persillier, M. D.	Montreal, P. Q.	1887
Landreth, Olin H., C. E.	Schenectady, N. Y.	1900
*Lartigan, J., M. D.	478 W. 59th St., New York, N. Y.	1899
Leal, John Lainy, M. D.	149 Ellison St., Paterson, N. J.	1891
Learned, John B., M. D.	Northampton, Mass.	1889
Lee, Benjamin, M. D.	1420 Chestnut St., Philadelphia, Pa.	1874
*Lehman, G. W., Ph. D.	City Hall, Baltimore, Md.	1900
Leiter, L. Z., Esq.	P. O. Drawer L, Chicago, Ill.	1876
Leighton, Marshall	Montclair, N. J.	1897
Lemp, William T.	St. Louis, Mo.	1884
Lewis, Daniel, M. D.	249 Madison Ave., New York, N. Y.	1894
Lewis, Richard H., M. D.	Raleigh, N. C.	1892
Licéaga, Eduardo, M. D.	4 San Andres St., Mexico, Mex.	1891
Lindsley, Charles A., M. D.	New Haven, Conn.	1875
Lopez, Fernando, M. D.	Mexico City, Mex.	1901
Lumsden, W. J., M. D.	Elizabeth City, N. C.	1892
*Lyons, Robert E.	630 East 3d St., Bloomington, Ind.	1900
Magennis, Bryan C., M. D.	Bridge St., Paterson, N. J.	1900
Marchand, Charles	57 Prince St., New York, N. Y.	1900
Marks, Solon, M. D.	Milwaukee, Wis.	1876
Marsh, E. J., M. D.	600 Park Ave., Paterson, N. J.	1872
Marshall, Charles E.	Agricultural College, Mich.	1901
*Matson, E. G., M. D.	6th Ave. and Cherry Alley, Pittsburg, Pa.	1900
Matthews, W. E., M. D.	425 Lincoln St., Johnstown, Pa.	1897
May, Wm. B.	251 Jefferson St., Buffalo, N. Y.	1901
McClelland, J. H., M. D.	5th and Wilkins Ave., Pittsburg, Pa.	1885
McClung, Hester	312 N. West St., Indianapolis, Ind.	1900
McCormack, J. N., M. D.	Bowlilng Green, Ky.	1880
*McDaniel, O., M. D.	Minneapolis, Minn.	1901
Macdonald, Jno. A., M. D.	Brandon, Manitoba.	1900
*McFarland, Joseph, M. D.	447 Stafford St., Germantown, Pa.	1897
McKenzie, Thomas H., C. E.	Southington, Conn.	1893
McKlveen, John A., M. D.	Chariton, Ia.	1899
*McLaughlin, Geo. E., M. D.	41 Crescent Ave., Jersey City, N. J.	1901
McNeil, J. Henry, M. D.	33 Sheboygan St., Fond du Lac, Wis.	1896
Mead, Daniel Webster, C. E.	1st National Bank Bldg., Chicago, Ill.	1892
Mead, Julian Augustus, M. D.	Irving St., Watertown, Mass.	1896

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Meehan, J. H., M. D.............................Niagara Falls, N. Y. 1901
Miller, Ashley..........................317 King St., Alexandria, Va. 1900
Mitchell, Henry, M. D.........................Asbury Park, N. J. 1886
Mohr, Charles, M. D.....................Oak Lane, Philadelphia, Pa. 1897
*Monjarás, Jésus E., M. D.............2 a de Yturbide No. 1, Mexico, D. F.
Monjeau, C., C. E................................Middletown, Ohio. 1887
Montgomery, L. H., M. D...................92 State St., Chicago, Ill. 1879
Montizambert, Frederick, M. D......................Ottawa, Ont. 1885
*Moore, Veranus A., M. D..........Veterinary College, Ithaca, N. Y. 1896
Morgan, David Percy.................Box 2036, New York, N. Y. 1885
Morris, Moreau, M. D...........109 E. 73d St., New York, N. Y. 1896
Mosher, Eliza M., M. D.............44 Court St., Brooklyn, N. Y. 1889
Mulford, Henry K...............412 So. 13th St., Philadelphia, Pa. 1896
Munson, E. L., M. D....Surgeon General's Office, Washington, D. C. 1901
Murray, Robert D., M. D., P. H. and M. H. S.....St. Augustine, Fla. 1872

Nagle, John T., M. D........163 West 126th St., New York, N. Y. 1874
Nash, Herbert M., M. D...........................Norfolk, Va. 1878
Neff, J. L., M. D...............Mutual Life Bldg., Philadelphia, Pa. 1897
Newbury Library.................................Chicago, Ill. 1901
Newell, F. H............U. S. Geological Survey, Washington, D. C. 1897
Newman, G. A., M. D.........................New London, Minn. 1899
New York Public Library......40 Lafayette Place, New York, N. Y. 1898
Nolte, Arthur, M. D.................1505 State St., New Orleans, La. 1901
*Noriega, Pedro, M. D..............................Monterey, Mex. 1901
Nunn, R. J., M. D..................5 York St. East, Savannah, Ga. 1881
*Nuttal George H. F., M. D.,
3 Cranmer St., Pathological Laby. New Museum, Cambridge, Eng. 1891

Ohage, Justus, M. D...........7th and Waconta Sts., St. Paul, Minn. 1899
Ologui, Augustin Ruiz, M. D............San Juan del Rio Qro, Mex. 1901
Orvanaños, Domingo, M. D..........25 Chavarria St., Mexico, Mex. 1890
Otis, Edward Osgood, M. D...........381 Beacon St., Boston, Mass. 1894
Otto, Jacob S., M. D................394 Frankllin St., Buffalo, N. Y. 1900
Owen, James, C. E...................................Montclair, N. J. 1901
Owen, W. O., M. D...............................Fort Thomas, Ky. 1900

Palmer, Arthur William........University of Illinois, Urbana, Ill. 1901
*Park, Wm. H., M. D................315 W. 76th St., New York, N. Y. 1899
*Parker, Horatio N..................................Montclair, N. J. 1900
Parmalee, Chas. L.................136 Liberty St., New York, N. Y. 1899
Parry, Eleanor, M. D....Mt. Holyoke College, South Hadley, Mass. 1901
Pearson, Leonard, V. M. D..........3608 Pine St., Philadelphia, Pa. 1897
*Pease, Herbert D., M. D.........116 Washington Ave., Albany, N. Y. 1897
Peck, George, M. D., U. S. N....926 No. Broad St., Elizabeth, N. J. 1890
Pennsylvania R. R. Relief Dept......................Trenton, N. J. 1899
*Perkins, Jay, M. D..............................Providence, R. I. 1899
Pierce, Imogene S..................................Manassass, Va. 1901
Pleasants, Henry.......................................Wayne, Pa. 1901
Pohl, Gustav, M. D...................96 Lemon St., Buffalo, N. Y. 1901
Porter, Joseph Y., M. D...........................Jacksonville, Fla. 1878
Powers, F. W., M. D...............................Reinbeck, Iowa. 1901

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Pray, Susan Remsen, M. D........135 So. Ninth St., Brooklyn, N. Y. 1896
Preston, C. H., M. D............1322 Fulton Ave., Davenport, Iowa. 1900
Prevost, Paul E., M. D..............................Montreal, P. Q. 1900
Probst, C. O., M. D................................Columbus, Ohio. 1886
*Prudden, T. Mitchell, M. D......160 W. 59th St., New York, N. Y. 1899
Putnam, Helen C................................Providence, R. I. 1901

Randall, G. M., M. D..............408 Middlesex St., Lowell, Mass. 1900
*Ravenel, M. P., M. D....University of Pennsylvania, Philadelphia, Pa. 1897
*Ravold, Amand, M. D...............................St. Louis, Mo. 1897
Raymond, Joseph H., M. D...L. I. College Hospital, Brooklyn, N. Y. 1882
Renner, Wm. Scott, M. D..............316 Pearl St., Buffalo, N. Y. 1901
Repp, John J., V. M. D...................................Ames, Ia. 1899
Reynolds, A. R., M. D.........................City Hall, Chicago, Ill. 1893
Reynolds, M. H., V. M. D...................St. Anthony Park, Minn. 1899
Richard, Charles, M. D.................................Manila, P. I. 1900
Richards, Edgar..................341 W. 88th St., New York., N. Y. 1889
*Richards, Mrs. E. H........Institute of Technology, Boston, Mass. 1888
Richardson, Maj. Henry B., C. E..................New Orleans, La. 1880
Richardson, W. L., M. D....225 Commonwealth Ave., Boston, Mass. 1875
Rickards, Burt Ransom..........607 Sudbury Bldg., Boston, Mass. 1901
Ridpath, H. W., M. D..........1603 College Ave., Indianapolis, Ind. 1900
Riley, Jno. A., M. D................1677 Greenshaw St., Chicago, Ill. 1901
Rives, William C., M. D...............1723 I St., Washington, D. C. 1885
Roberts, Charles F., M. D........250 W. 57th St., New York, N. Y. 1892
*Robin, Albert, M. D..............817 Adams St., Wilmington, Del. 1899
*Robinson, Prof. Franklin C........................Brunswick, Me. 1889
Robinson, John Franklin, M. D.................Manchester, N. H. 1894
*Rosenau, M. J., M. D..........P. H. and M. H. S., Washington, D. C. 1901
*Russell, H. L...Madison, Wis. 1899
Russell, Julia Wallace, M. D.......44 Pleasant St., Concorn, N. H. 1892

Salmon, D. E., D. V. M.........................Washington, D. C. 1883
Schleur, Wm. H.......................................Orange, N. J. 1900
Schoonover, Warren, M. D....115 East 59th St., New York, N. Y 1895
Scott, Walter A., M. D..........610 Pine St., Niagara Falls, N. Y. 1901
Seaman, Louis A., M. D...............18 W. 31st St., New York, N. Y. 1901
Shea, Thomas B., M. D............Old Court House, Boston, Mass. 1900
Shepard, Charles H., M. D......Columbia Heights, Brooklyn, N. Y. 1892
Shrader, John C., M. D...........................Iowa City, Iowa. 1890
Shulz, F. W., M. D....................663 16th St., Milwaukee, Wis. 1901
Simon, F., M. D..........................336 Spring St., Buffalo, N. Y. 1901
*Smart, Charles, M. D., U. S. A..................Washington, D. C. 1882
Smart, G. I., M. D..............................Blue Earth, Minn. 1899
Smersh, F. M., M. D.............................Owatonna, Minn. 1899
*Smith, Erastus G., Ph. D..............................Beloit, Wis. 1901
Smith, Frederick W., M. D........700 S. West St., Syracuse, N. Y. 1896
*Smith, Herbert E., M. D., Med. Dept., Yale Univ., New Haven, Conn. 1899
Smith, Stephen, M. D..........640 Madison Ave., New York, N. Y. 1872
*Smith, Theobald, M. D..................Forest Hills, Boston, Mass. 1899
Smith, T. Guilford............9 German Ins. Bldg., Buffalo, N. Y. 1896

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* Also Members of Section of Bacteriology and Chemistry.

Smith, Wm. F., M. D..........................San Juan, Porto Rico. 1901
Smock, Prof. John C..................................Trenton, N. J. 1899
Snow, F. Herbert, C. E..............................Boston, Mass. 1900
Soper, Geo. A., Ph. D................29 Broadway, New York, N. Y. 1900
Spaulding, Herman, M. D...................City Hall, Chicago, Ill. 1900
Spinning, Alva L., M. D......219 E. Market St., Michigan City, Ind. 1900
Standlee, Eli L., M. D..........2201 University Pl., St. Louis, Mo. 1895
Stanton, Byron, M. D.............931 Dayton St., Cincinnati, Ohio. 1889
Staples, Franklin, M. D...........................Winona, Minn. 1885
*Stebbins, James H., Ph. D........80 Madison Ave., New York, N. Y. 1900
*Stengel, Alfred, M. D.........Pepper Laboratory, Philadelphia, Pa. 1899
*Sternberg, George M., M. D., U. S. A.............Washington, D. C. 1874
Stillson, H. L., M. D..............................Bennington, Vt. 1899
*Stokes, Wm. R., M. D..............................Baltimore, Md. 1899
*Stone, Bingham H., M. D..........................Burlington, Vt. 1900
Suiter, A. Walter, M. D...........................Herkimer, N. Y. 1894
Suiter, F. C., M. D...............................La Crosse, Wis. 1899
Sutherland, Quincy Orlin, M. D..73 Milwaukee St., Janesville, Wis. 1895
*Swarts, Gardner Taber, M. D....................Providence, R. I. 1894
Syracuse University...............................Syracuse, N. Y. 1899

Talbot, Miss Marion...........University of Chicago, Chicago, Ill. 1893
*Teichmann, Wm. C....................1141 Market St., St. Louis, Mo. 1901
Thau, A. Blair, M. D............Park Row Bldg., New York, N. Y. 1901
Townsend, Joseph H., M. D........39 College St., New Haven, Conn. 1901
Traver, Lorenzo, M. D.............5 Stewart St., Providence, R. I. 1892
Trenner, Simeon.................................Norristown, Pa. 1901
Tuell, Jas. S., M. D...................222 Water St., Augusta, Me. 1901
Turnbull, Thomas, M. D..........314 Western Ave., Allegheny, Pa. 1897

Utley, Edward R., M. D..............................Newton, Mass. 1898

Valentine, Fred C., M. D..........31 W. 6th St., New York, N. Y. 1896

Wade, John W., M. D................318 No. 2d St., Millville, N. J. 1897
Walcott, Henry P., M. D.......................Cambridge, Mass. 1880
Walker, Charles R., M. D..........................Concord, N. H. 1894
Walsh, Ralph, M. D............................Washington, D. C. 1885
Ward, L. B., C. E............702 St. Nicholas Ave., New York, N. Y. 1885
Warner, Frank, M. D..................................Columbus, O. 1899
*Wasdin, Eugene, M D.............U. S. M. Hospital, Buffalo, N. Y. 1901
Watson, Irving A., M. D..........................Concord, N. H. 1881
Welch, William M., M. D......821 No. Broad St., Philadelphia, Pa. 1897
Wende, Ernest, M. D................296 Water St., Buffalo, N. Y. 1894
Wende, Jno., M. D...................1593 Main St., Buffalo, N. Y. 1901
*Wesbrook, Frank F., M. D......................Minneapolis, Minn. 1897
*Weston, Robert Spurr................14 Beacon St., Boston, Mass. 1896
*Whipple, Geo. C.,
Flatbush Ave. and Eastern Parkway, Brooklyn, N. Y. 1899
White, Alfred T., M. D..................Box 619, New York, N. Y. 1899
White, William A., M. D................Box 619, New York, N. Y. 1888
Whitney, Eli...................................New Haven, Conn. 1900

* Also Members of Section of Bacteriology and Chemistry.

Wilbur, Cressy L., M. D..........................Lansing, Mich. 1894
*Wiley, H. W................1314 10th St. N. W., Washington, D. C. 1901
Williams, Herbert A., M. D..............24 High St., Buffalo, N. Y. 1901
Wilkinson, C. P., M. D..........1402 Peters Ave., New Orleans, La. 1889
Willcox, W. F.....................Cornell University, Ithaca, N. Y. 1899
*Wilson, Ezra H., M. D......Hoagland Laboratory, Brooklyn, N. Y. 1897
*Wilson, Robert, Jr., M. D........................Charleston, S. C. 1901
Wingate, Uranus Owen Brackett, M. D.,
204 Biddle St., Milwaukee, Wis. 1890
Wishard, William N., M. D.......................Indianapolis, Ind. 1901
Witts, M. J. M. D..................................Burlington, Vt. 1901
Wood, Alfred Vincent..........1777 Gloucester St., Brunswick, Ga. 1892
Wood, Mrs. C. P....................108 Powell Ave., Evansville, Ind. 1900
Wood, Prof. Edward S......Harvard Medical School, Boston, Mass. 1895
Woodbridge, Prof. S. H.............................Boston, Mass. 1895
Woodcock, G. M., M. D..................................Bangor, Me. 1893
Woodhull, Col. A. A., M. D., U. S. A..............Princeton, N. J. 1874
Woodward, R. M., M. D.......P. H. and M. H. S., Washington, D. C. 1901
Woodward, William C., M. D...................Washington, D. C. 1896
Worcester Free Library.........................Worcester, Mass. 1897
Wright, Frank W., M. D..........48 Pearl St., New Haven, Conn. 1889
Wright, John W., M. D.......................................Erie, Pa. 1897
Wyman, Walter, M. D........P. H. and M. H. S., Washington, D. C. 1884

Young, A. G., M. D..................................Augusta, Me. 1885
Youngerman, Charles, M. D.....................Williamsport, Pa. 1901

HONORARY MEMBERS.

Ex-President Grover Cleveland...................Princeton, N. J. 1885
J. B. Russell, M. D., LL.D.......................Glasgow, Scotland. 1886
Mrs. Elizabeth Thompson........................New York, N. Y. 1887
President Porfirio Diaz ....Mexico, Mex. 1892

LIFE MEMBER.

Henry Lomb ....Rochester, N. Y. 1884

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* Also Members of Section of Bacteriology and Chemistry.

MEMBERS ELECTED AT NEW ORLEANS MEETING, DECEMBER 9-12, 1902.

Adams, J. J...New Orleans., La.
Agnew, Frank E., M. D..................................Paterson, N. J.
Allen, Thomas G., M. D..................439 Elk St., Buffalo, N. Y.
*Archinard, John J., M. D............830 Canal St., New Orleans, La.
Archinard, Paul Emile, M. D........830 Canal St., New Orleans, La.

Barrow, A. Feltus, M. D...............................St. Francisville, La.
Beyer, George E................Tulane University, New Orleans, La.
Bland, John J., M. D..........1043 Carondelet St., New Orleans, La.
Brown, George, M. D...Atlanta, Ga.
Brownfield, James H., M. D........321 Main St., Fairmont, W. Va.
Butterfield, F. C...Derby Line, Vt.

Campbell, Edw. R., M. D..................................Bellows Falls, Vt.
Cantrell, Geo. M. D., M. D......116 Louisiana St., Little Rock, Ark.
*Carey, Harry Wardell, M. D......22 Lancaster St., Albany, N. Y.
Carter, Henry R., M. D.......U. S. Marine Hospital, Baltimore, Md.
Chacon, Augustin, M. D.......................................Mexico, Mex.
Chalaron, Frank J., M. D......1318 Esplanade St., New Orleans, La.
Crossland, Jefferson C., M. D........213 Seventh St., Zanesville, O.

Dalrymple, Wm. H., M. D..................................Baton Rouge, La.
Dyer, Isadore, M. D..............124 Baronne St., New Orleans, La.

Earl, George Goodell........602 Carondelet St., New Orleans, La.
Eliot, Llewellyn, M. D........1106 P St. N W., Washington, D. C.
Finlay, Carlos Juan, M. D........150 Campanario St., Havana, Cuba.
Fogarty, Joseph Norman, M. D........227 Duval St., Key West, Fla.
Folkes, Hyman McMackin, M. D........Howard Ave., Biloxi, Miss.

Garcia, Eduardo, M. D..........Santiago de Chili, Republic of Chili.
Gayon, José P., M. D......................................Mexico City, Mex.
Greene, Walter D., M. D..............385 Jersey St., Buffalo, N. Y.
Grubbs, Samuel Bates, M. D....................Washington, D. C.
Guiteras, Juan, M. D..............119 San Miguel St., Havana, Cuba.
Guthrie, John B., M. D............124 Baronne St., New Orleans, La.

Hawkins, R. G., M. D...Palmetto, La.
Hurtado, Francisco, M. D...1636 a Presidente St., Mexico City, Mex.
Haase, Marcus, M. D......................................Memphis, Tenn.
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